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INTRODUCING A NEW WAGE STRUCTURE INTO COAL-MINING

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Introduction
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In April 1955, a new national wage structure for daywagemen was introduced in the coal-mining industry of Great Britain. The reform of the piece rate structure, work on which is in progress, will it is hoped be the subject of a separate article.

The interpretations and opinions expressed here are not necessarily shared either by the National Coal Board or by the National Union of Mineworkers.

INTRODUCTION

Those who wish to test economic or any other theories against the realities of a wage structure, be it the wage structure of a particular industry or of the country as a whole, are immediately confronted by the serious handicap of lack of data. Those who are called upon to manipulate the wage structure of an industry find themselves in a similar difficulty. It is not easy to obtain reliable data which show, in the requisite detail, what the wage structure of a particular industry looks like in reality; and it is not safe to rely on figures of average earnings for selected occupations or for broad divisions based on levels of skill.

Recent articles in the BULLETIN have thrown light on the true structure of wage rates and earnings in a number of industries. This article may help to fill a similar gap in respect of the wage structure of manual workers in the coal-mining industry.

Problems of wage policy have many aspects; for example, general wage levels, systems of remuneration, and wage structure. For any particular industry, the first group of problems has to do with determining the size of the wage bill, and with the raising or lowering of general wage levels. Questions of this kind, however, are outside the scope of this article. This is fortunate: for one eminent economist confessed to have suffered a distressing nightmare when dreaming of being called upon to give advice and guidance on such matters.[1] Another has written a book recording her search for 'principles '.[2] Regarding systems of remuneration in the coal-mining industry, no thorough evaluation of the efficacy of different systems of remuneration has yet been conducted. The main concern of this article is, therefore, with the problem of wage structure—which, fundamentally, is a problem of distributing a wage bill of a given size among workers in the different occupations of an industry.

I. THE BACKGROUND

Collieries

That part of the coal-mining industry operated by the National Coal Board consists of about 850 separate collieries, employing in all some 694,000 industrial workers. In addition there are some 536 ' small mines ' operated under licence by private owners. By the terms of the licence, the wage agreements concluded between the Board and the N.U.M. are also applicable to these mines. However, the information used in this article refers only to Board mines.

The 850 separate collieries are scattered over many different parts of the country ranging from Kent in the South-East, and Bristol and Somerset in the South-West, to Scotland in the North. For wage purposes, these collieries fell into 20 separate ' wage districts ', but even within the same coalfields they exhibit remarkable diversity in almost every characteristic imaginable,[3] size, age, life, degree of mechanisation, thickness of seams, output, productivity, costs, absence rates, incidence of strikes and so on. The distribution, in January 1956, of Board collieries and mineworkers by wage districts is shown in Table I.

Industrial workers

Industrial workers at collieries are by no means all manual workers. Three main categories are customarily distinguished; first there is a main body of manual workers, including working chargemen; second, there are the ' line ' officials below the undermanager, collectively styled under-officials and consisting of Overmen, Deputies and Shotfirers. Finally, there is a mixed group of ' staff ' known in coal-mining as ' Weekly Paid Industrial Staff '. For the most part they consist of various ' levels ' of Foremen, together with specialists such as Safety Officers, Training Officers, Ventilation officers, Dust Suppression Officers and so on. The relative size of these categories is shown in Table II.

Although the wage structure of each of these categories has undergone radical changes since 1952, this article is mainly confined to the problems

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[1] Sir Dennis Robertson: The 1954 Stamp Memorial Lecture on *Wages.*

[2] Barbara Wootton: *The Social Foundations of Wage Policy* (1956).

[3] *cf.* Sir Hubert Houldsworth: ' The Pits of Britain ', *Manchester Statistical Society,* Feb. 1953; W. H. Sales: ' Changing Patterns of Pit Performance,' *The Journal of Industrial Economics,* July, 1955.

TABLE I

Location of Coal-mining Industry, January 1956

Wage district	*Number of pits*	*Industrial workers on Colliery Books*
Scotland	171	82,780
Northumberland	59	41,441
Cumberland	9	5,584
Durham	130	101,496
North Eastern	111	137,764
Lancs. and Cheshire	62	48,940
North Wales	6	8,844
Nottingham	37	86,729
North Derby	36	
South Derby	8	14,593
Leicestershire	8	
North Staffs	20	19,720
Cannock Chase	14	15,272
South Staffs	7	5,025
Shropshire		
Warwickshire	11	15,681
South Wales	143	98,177
Forest of Dean	6	2,575
Somerset	8	2,989
Kent	4	6,857
Great Britain	850	694,467*

* In the same period the corresponding figure of 'effective' industrial workers was about 635,000.

TABLE II

Category	*Approximate number of employees on Colliery Books, April, 1956*
Manual Workers (including Chargemen)	647,400
Under-officials	36,200
Weekly Paid Industrial Staff	6,900

involved in revising the wage structure of manual workers, numerically the largest group in the industry.

Manual workers

Manual workers may also be sub-divided into important sub-groups. First, there is the distinction based on sex. Although coal mining is primarily a male industry, a small number of women is employed, the main concentration being in Lancashire and Cheshire. Second, there is the distinction based on age. With minor exceptions, only workers aged 21 years and over are regarded as adults for wage-payment purposes. Below 21, the principle of a rate for age applies. Approximately 10 per cent of manual workers in coal mining are under 21 years of age. In the remaining sub-groups there are workers employed at Central Workshops, which are responsible for servicing, maintaining and repairing colliery machinery; there are also colliery surface workers and colliery underground workers. In effect, these groups represent important substructures, their numerical strengths being given in Table III.

TABLE III

Manual Workers on Colliery Books, April 1956

Location	*Males*		*Females*		*Total*
	21 & over	*under 21*	*21 & over*	*under 21*	
At Central Workshops ...	6,941	1,277	6	2	8,226
At Collieries:					
surface	118,993	12,040	346	27	131,406
underground	460,331	55,658	—	—	515,989

Another way of classifying manual workers is by the system of remuneration. Broadly, three systems find widespread use in coal-mining. First, there is payment by time alone. This is the most common system, some 57 per cent of *manual* workers being time workers. Then there is piecework, the most common variety of which is straight piecework; this applies to about 40 per cent. Finally there is a small number of taskworkers, who are paid a comparatively high task rate (usually about the level of the average earnings of pieceworkers less Five Day Week Bonus) plus some form of incentive bonus. This system finds most popularity with teams who work power loading machines.

Development of manual workers' wage structure

The wage structure of the coal-mining industry has been built up locally at each colliery. Previously each wage rate was compounded from two main elements—a *basis* rate fixed locally at each colliery for a particular job, and a percentage addition. The function of the basis rate was to cater for differences in the grade of job and for varying physical conditions under which the job was performed. The percentage addition had a different function. Its value was determined over large areas or wage districts, but differed from district to district. It acted as a barometer for wage levels, fluctuating with changes in the prosperity or economic condition of particular areas or wage districts. Regular ' wage ascertainments ' were carried out to determine the amount of the percentage addition, which in turn virtually decided the wage levels that were to apply until the next ascertainment. Changes in basis rates could still, however, take place at particular collieries, usually in response to changes in physical conditions, new types of machinery or new methods of working. But as a general rule these changes were few, and basis rates remained fixed over comparatively long periods of time. Furthermore, individual firms could and did supplement basis rates and percentage additions by giving bonuses and other forms of ' allowances '. An additional feature of this system was some kind of ' district minimum wage ', usually comprising a minimum percentage addition to basis rates.

This system based on ' district ascertainments ' lasted until 1944, but had been modified from time to time by the operation of national agreements: in particular, by the 1940 War Additions to Wages Agreement, more popuarly known as the Cost of Living Agreement. It was designed to meet changes in the cost of living, and was based on the index then prevailing. Uniform flat-rate advances to all workers were superimposed on the wage levels determined by the ascertainments. Another modification, introduced in 1942, was the national weekly minimum wage for surface and for underground adult workmen. Its effect was to telescope differentials between many jobs at the lower end of the scale, the telescoping being more severe in the lower-paid districts. Thus it not only narrowed the gap between the lowest- and highest-paid men (more in some districts than in others), but it also tended towards greater equality in average wage levels between wage districts. Shortly before the district wage ascertainments system was discontinued by national agreement in 1944, a national weekly minimum rate-for-age scale was introduced for persons under 21 with similar effects on the various wage districts. When the district ascertainments system was discontinued in 1944, the prevailing percentage and flat-rate additions were merged. They were then frozen for a minimum period of 3½ years, except for changes required by new or different methods or conditions of working and by the operation of the 'War Additions to Wages Agreement.' Certain skilled workers were also given a 1s. per shift flat-rate increase which was not consolidated.

With the advent of nationalisation, the National Coal Board took over the agreements which had shaped the wage structure up to that time. However, with the introduction of the Pit Conciliation Machinery in January, 1947, applications for changes in piece rates at pit level were permitted, thus lifting the restriction imposed by the 1944 Agreement. This meant that, while the wages of nearly all timeworkers continued to be regulated by national agreements (local changes for timeworkers being limited to those introduced as a result of changed methods and conditions of working), pieceworkers could seek changes at pit level. Since then, and apart from the effect of changes in the level of the minimum wage and of the Five Day Week Agreement, pieceworkers have with one exception continued to use this mode of raising their piece rates. The exception occurred in November 1951, when pieceworkers participated in a national wage increase in return for a standstill on local changes in piece rates for a period of 12 months.

The Five Day Week Agreement introduced in May 1947 did not alter the pattern of the basic wage structure. However, it did superimpose an 'attendance bonus' on the structure prevailing at that time, as well as standardising the normal working hours. From 1st January 1947 to 4th April 1955, when the new wage structure was introduced, there were 6 national flat-rate increases to timeworkers, all of which followed much the same pattern. National weekly minima were raised and the general increases were limited at the upper end of the scale by 'ceilings' which tapered off the increases. This succession of *pro tanto* flat-rate increases did to some extent narrow the gap between the lowest- and highest-paid workers, but contributed very little to rationalising wage rates for similar grades of jobs over the country as a whole. *General* increases of this kind do not discriminate between occupations sufficiently to be regarded as an effective instrument for ironing out anomalies. To do this effectively, the *pro tanto* method must be made more selective and specific to particular occupations or grades.

The contribution of this succession of national, and for the most part uniform, flat-rate increases to a more coherent national wage structure was small. Perhaps the main result was the introduction of national weekly minima, although these wiped out many long standing local and district inter-job differentials at the lower end of the wage-ladder. The ' ceilings', too, had caused some compression at the upper end. Between 1944 and 1955, only two groups of workers—craftsmen and winding enginemen—had national rates fixed for them (1948). These comprised about 10 per cent of all time-workers, or some 6 per cent of all workers. For all workers except the two groups mentioned, only minima were fixed nationally; and, as will be seen, the skeleton of national minima gives a very inadequate picture of the shape of the occupational pattern of wage rates.

Before we leave this brief historical sketch, there is one point which ought to be stressed. From 1944 onwards, *local* changes in time rates (as well as changes in district rates) were virtually prohibited, on the principle that local changes on any scale would be inconsistent with national flat-rate advances in wages. This embargo was not observed to the letter, but it certainly prevented all but a trickle of local increases.

Need of adequate data

There was very little precise knowledge about the pre-1955 wages structure for manual workers. As for actual wage rates for each occupation at the Board's 850-odd collieries, no one quite knew how many distinctive occupations there were, let alone the manner in which wage rates were distributed in each of them. Of course, the structure was known to contain many marked and inexplicable differences in wage rates for the same occupations even within limited areas, but unless reasons can be advanced to explain differences in wage rates, the grounds for maintaining them are widely felt to be irrational and unjust. This is perhaps only fully appreciated by persons who are continually faced with strong pressure for removing differences. Unless there is some mutually agreed framework, or set of principles to which appeal can be made, the situation very soon degenerates into power politics and horsedealing.

Experience of the problems thrown up by national wage increases had made it clear that a new wage structure could not be created by inventing expedients to deal with anomalies, or by patching up the structure here and there. The existing system had to be dismantled and put together again in a more rational manner. But before that could be done, the system had, for the first time, to be examined in detail and as a whole. Much more precise and detailed anatomical knowledge had to be made available for a systematic reconstruction of the wage structure to be attempted with any confidence that it would work in practice.

The first major problem was, therefore, how best to get a comprehensive and accurate picture of the wage rates paid in the large number of ostensibly different occupations in coal mining.[1] Except for a few occupations, such information had not previously been collected. For the purpose of conferring flat-rate increases all round, detailed information is of course not needed. But for remodelling the wage structure a frequency distribution of wage rates for each occupation was essential. Without such basic data, possible new wage structures could not be designed, nor could their characteristics be analysed and some of the consequences of introducing them be predicted. It would have been foolhardy to attempt to carry out the radical changes required without knowledge of what the wouldy be likely to involve.

II. The Framework

Standardising occupations

The problem of defining the various occupations in coal-mining was the first hurdle. No one knew how many occupations existed, and it was uncertain that any named occupation entailed the same sort of work at different collieries. Sometimes the same names described radically different jobs. Unless there was reasonable certainty that an occupation always embraced more or less the same sort of work, no confidence could be placed in frequency distributions of occupational wage rates.

The first step was therefore to find out exactly what jobs were being done by the workmen in the industry. Every colliery in the country was asked to compile a list of its jobs, giving the local title and a brief job description of the work involved. Although a colliery of medium size usually has only about 80 to 100 different jobs done by manual workers, the returns showed that there were apparently over 6,500 different jobs in existence in the industry as a whole. Closer examination revealed that many of these ' jobs ' were in fact only local names describing the same work. Picturesque titles such as ' windy driver ' for a compressed air engine driver, ' wreck-watcher ' for the job of patrolling underground roadways for derailed tubs, ' snecker ' for the job of operating pit top cage catches, and ' beetle lad ' for a haulage hand, are but a few of the more colourful examples of the many local names. But to add to the confusion, identical local names were sometimes found to describe different jobs. For instance, the term ' haulier ' in South Wales denoted a horsedriver; in South Staffordshire and Durham it could mean a haulage engine driver, or even a shackler of tubs.

The reasons for this multiplicity of names are many and varied. In the first place, coal-mining has a long history. Even if the ' modern ' mining era is dated from about 1800, there is a span of over 150 years in which hundreds of collieries, some little more than holes in the ground, others involving a capital expenditure of £7 millions or more, have been sunk and developed. Many of them have been, or are being, rejuvenated. An old pit, however, frequently means antiquated systems of layout and methods of mining, and this will often show itself in the types of jobs done. A colliery working coal on the hand got system with pony haulage will have some jobs not to be found in a modern fully mechanised mine. Secondly, collieries extend over widely scattered coalfields and over quite substantial areas within the same coalfield. Philologists encounter some quite remarkable variations in the nomenclature of similar objects even within such a relatively small geographical area as the British Isles. It is the same with collieries; the same job goes under different names according to the coalfield. For example, the job of coal getting (only about one-seventh of colliery workers actually do this) is done by workmen known variously as 'colliers', 'strippers', 'hewers' and 'getters'. Occupations found at almost every colliery are sometimes known by names which are current only in particular coalfields. Thus 'rippers'—the men who drive the tunnels or 'gates' giving access to the coalface—become 'brushers' in Scotland; 'hitchers' in South Wales—men who push tubs or trams into cages at the pit bottom—become 'onsetters' in Lancashire and elsewhere; and so on. Thirdly, collieries were until recently under different ownership. There was no special need for standardisation and no premiums on accuracy of job titles. Under such conditions, it would have been remarkable if there had emerged any close association between work content and job title, even over small coalfields.

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[1] The Registrar General's *Classification of Occupations* for the 1951 Census listed over 1,000 of these in coalmining, the bulk of which were manual jobs. Actual examination produced a list at least six times as long.

At an early stage, it became clear that a reliable picture of the prevailing wage structure could not be obtained unless it could be related to a common framework. This meant producing a standard list of job descriptions or occupations together with their titles. The preliminary work of sorting and grouping all the occupations was perhaps the most laborious and painstaking task undertaken, and calls for some account of the way in which it was tackled. First, jobs which were identical in work content, irrespective of title, were identified. Then jobs which were not quite identical, but were nearly so, were added and the related job description was suitably expanded. This process of grouping was continued until all jobs whose 'main content' was identical, minor differences being ignored, had been brought together and given a common job description. To this was added a 'national' title and against it were listed all the local names or titles belonging to that particular occupational group.

This preliminary compression and simplification was by no means straightforward. The task of seeking identities in a long list of jobs, and of arranging them systematically in groups, took many months. The first standard list of occupations was sent to the coalfields for checking and to find out what modifications were required. Once this stage had been reached, the co-operation of the National Union of Mineworkers was sought. A joint committee was set up to discuss the tentative standard list of job descriptions: each description was subjected to searching scrutiny, further changes were made and after many weeks there emerged an agreed national list of jobs and job descriptions. Without this framework—this agreed national dossier of jobs—further advance would have been difficult if not impossible. The scale and complexity of this phase of the exercise may be judged from the fact that it took more than a year to complete, and resulted in a standard list of some 350[1] national jobs and job descriptions condensed from the original list of over 6,500. This compression, simplification and standardisation were essential to the next step—the collection of the necessary frequency distributions of wage rates for each occupation.

Defining a ' wage rate '

Before the collection of frequency distributions of wage rates could proceed, however, other difficulties of definition had to be resolved. At first glance the mere collection of frequency distributions of wage rates would not appear to be very difficult. However, to define unequivocally what was meant by a ' wage rate ' turned out to be far from straightforward. The term as used in coal mining could be given a number of different interpretations, and care had to be taken to ensure that every colliery applied the term in a standard manner. The ' wage rate ' was eventually defined as the money paid to a workman for an ordinary shift's work, excluding the Five Day Week Bonus. The latter was excluded because it is not automatically paid for each shift worked, but depends on the number of shifts worked in a normal week, and on certain approved reasons for absence when there is failure to attend on the full number of normal shifts. Similarly, the money earned in overtime was excluded. On the other hand, all flat-rate additions, allowances or bonuses—local or national—were included, since it was intended eventually to consolidate them. There were a large number of local allowances, masquerading under all sorts of names, although often there was no explanation of their origin. The tendency for allowances, once made, to be self-perpetuating long after the conditions which originally gave rise to them have disappeared is a well-known phenomenon. One of the trickiest jobs of revising the wage structure was to clean up these local allowances and to streamline the time rates.

Some idea of the difficulty of this operation may be got from the fact that nearly a quarter of the final agreement was devoted to the manner in which this ' streamlining ' of the wage rate was to be carried out. From the outset it was necessary not only to define what was meant by a wage rate but also to specify the allowances which were to be consolidated and, therefore, to anticipate in advance the precise manner in which allowances would eventually be consolidated. National flat rates such as the ' skilled shilling ' (paid for every shift worked) were straightforward. But intermittent allowances were more difficult to deal with; and the existence of the national weekly ' cash and kind ' minima meant that detailed instructions on the treatment of ' make up ' had to be issued to ensure uniformity of treatment.

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[1] Subsequent changes brought this figure up to about 400.

Collecting data—the ' trial assimilation '

Once the occupations had been standardised and the wage rate defined, it was necessary to know how many men were employed in each job, and what wage rates they were being paid. Without such data it would have been impossible to discover what changes were required in wage rates, how much these would cost, how many men would be affected and how much their wage rates would increase or decrease, or the degrees to which men, collieries, wage districts or divisions would be affected. We therefore carried out a ' trial assimilation '. In the first instance, this was limited to daywage workers.[1] Colliery managements were asked to assimilate, that is to classify, all manual workers who were timeworkers into the appropriate national jobs and to record their current wage rates (as defined above). They were requested to discount any bias introduced by personal qualities of particular men, or by the existing levels of their wage rates. A return was thus obtained of the job structure of each colliery showing wage rates arranged by national jobs. The returns were to record only ' effective ' timeworkers (*i.e.* those paid wages during the chosen week), so as to give a more accurate basis for estimating the cost of any proposed changes.

The general picture conveyed by this enquiry is shown in Table IV. To complete the picture, data taken from subsequent surveys on pieceworkers have been added. The figures exclude females and persons under 21, who number about 65,000 effectives, as well as weekly paid industrial staff and under-officials numbering about 40,000. The main difficulty was that of handling the mass of material. For each job, and later for each grade, frequency distributions of different wage rates were constructed. A typical colliery would have between 80 and 100 different jobs, and thus provide as many frequency distributions. The colliery distributions were next aggregated by wage districts (of which there are 20), and then into national frequency distributions. Finally, the job frequency distributions were amalgamated by 13 grades.[2]

TABLE IV

Effective Adult Manual Workers (including Craftsmen)

Place of work	*Number of national jobs*	*Number of men* daywagemen	*Number of men* pieceworkers
Coalface	44	39,235	194,357
Elsewhere underground ...	92	141,626	39,252
Surface	125	108,483	1,617
Central Workshops	96	5,451	N.A.
Total	357	294,795	235,226

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[1] The problems involved in revising the piecework structure were in themselves so different in kind, and so complex, as to make it impracticable to do both simultaneously. Although the standard list of occupations applies equally to pieceworkers, the work described in subsequent paragraphs affects pieceworkers only indirectly, and it makes no frontal attack on the central problems of the piecework structure.

[2] *cf.* pp. 212-3 below.

Deployment of men in jobs

The first striking feature was the extreme skewness of the distribution of jobs. This is illustrated in Table V. At one extreme, 83 per cent of the men fell within 24 per cent of the jobs; at the other, over half the jobs accounted for only 3 per cent of the men. However, some numerically small jobs, such as that of winding engineman (3,672), are found at almost all collieries, while some jobs are confined to certain collieries only.

TABLE V

Daywagemen Elsewhere Underground

Number of men in given jobs	*Percentage of jobs*	*Percentage of men*
Under 50	25	0.4
50 and under 200	27	2.6
200 and under 1,000	24	14.0
1,000 and over	24	83.0

Wage-rate patterns and differentials

The frequency distributions of wage rates within occupations were examined with the expectation that they would throw light on such questions as: how far were different rates being paid for the same jobs? how far were these differences limited to certain wage districts? was it possible to distinguish clear-cut differences in wage levels for the same occupations between collieries in the same wage district, or between wage districts?

For the majority of jobs, the frequency distributions of wage rates followed much the same general pattern—for collieries, for districts, and for the country as a whole. Naturally, the distributions for the less populous jobs were more ragged, as were the district distributions compared with the national. But the pattern was nearly always the same, with a bunching of men towards the lower end of the wage scale. With few exceptions, the concentration of men occurred at or very near the national weekly minima. The lowest wage levels at which this bunching took place were, however, appreciably above the national floor in some of the wage districts—notably in the East Midlands Division. Although minor differences in the shape of the distributions, such as the emergence of secondary modes representing district influences, and slight differences in the means, could be detected, these differences were completely overshadowed by the extensive overlapping between the majority of jobs within the between collieries and districts. In short, there appeared to be no consistent clear-cut differentials,[1] even between jobs intuitively expected to possess distinctive levels of remuneration.

[1] The concept of a wage differential as a difference between two wage rates belonging to the same or to different occupations is quite straightforward. But it becomes more elusive and difficult to apply when overlapping frequency distributions are encountered. A previous article in the BULLETIN (Nov.—Dec. 1954) ' On the Difficulties of Measuring Wage Differentials '—Knowles and Hill—exposes these difficulties and suggests ways around them.

It is interesting to speculate why time rates in coal-mining should be distributed in this manner, whereas the frequency distributions of piece-workers' earnings (shorn of the effect of overtime and attendance) are much more symmetrical and bell-shaped. National profiles of time rates resemble in some ways the Pareto distribution. The hypothesis that such patterns are partly the product of random influences irrespective of merit or skill may not be completely without foundation. Some degree of randomness in the wage structure had, of course, been expected, but not to the extent revealed by the data. There were considerable inconsistencies in the wage rates paid to men doing identical jobs, even at individual collieries. Generally accepted ideas that some jobs had uniform rates over limited areas, ideas stemming from the fact that some district agreements prescribed rates for such jobs, had to be abandoned. The scatter of wage rates was such that some jobs, which by common consent should have had the lowest remuneration, were shown to have wage rates higher than those possessed by jobs at the top end of the scale. It is difficult to credit that such situations arise mainly as responses to economic stimuli; it seems more plausible to attribute them to the subjective assessments and preferences of management, unrestrained—at least within wide limits—by market considerations.

Job evaluation and grading

The absence of consistent differentials between jobs meant that no structure was available as a basis on which to build, and that a new wage structure would have to be invented. A new structure would moreover mean radical changes, which would disturb the prevailing pattern of relative wage rates on the widest scale.

What sort of pattern should be chosen and on what principles should it be based? If the results were to be acceptable in practice, these questions required joint decisions by the Board and the National Union of Mineworkers. Consequently, it was decided to set up a Joint Committee to work out a solution to the problem. The Committee decided that jobs of approximately equal ' value ' should be grouped into grades. This meant an evaluation of the jobs in non-monetary terms, by a systematic application of a set of criteria for assessing the relative ' worth ' of each job. There are many factors which could, ideally, contribute to this assessment; physical effort, danger, skill, responsibility and training are some of them. Weights for each of these features can be assessed and, by using a points system, a measure of consistency in job evaulation can be achieved. The alternative is to consider the job as a whole rather than to break it down into its constituent factors; and the Joint Grading Committee chose this method of evaluating the national jobs. It may be argued that the constituent-factor approach is the more likely certain to produce objective results, but this is not necessarily the case when the number of grades is small. Both methods involve a measure of subjective appraisal. The method of examining the job as a whole and assigning it to one of a few grades by common agreement produced results which in this case, were substantially confirmed by an independent job evaluation based on a points system. What matters is that the evaluation should be carried out systematically. Most people are willing to recognise two extreme classes of jobs—one of much and one of little ' worth '; they as readily willing to recognise a ' middle class '. It is only one step further to admit two additional classes, one between each extreme and the middle, thus giving five classes or grades in all. The National Grading Committee took *craftmen's* jobs out of the list and divided them into three grades; they then put the *underground* jobs into five grades, and the *surface* jobs into five grades. In this way the Committee found little difficulty in reaching agreement on classifying the 350 or so national jobs into 13 grades. There is of course nothing sacrosanct about a particular grouping and nothing strange about altering it by joint agreement. That it achieves on an industry-wide scale what a single employer may do (with or without the agreement of a Trade Union) for a particular establishment does not mean that a national system of grading of jobs need necessarily be insensitive to changing circumstances, economic or otherwise.[1]

The old pattern of wage rates related to the new grades

The framework of the new wage structure had now been constructed. The next step was to discover how the prevailing wage rates fitted the new grade structure. (This is shown in Table VII below.) The failure to discover clear cut differentials between jobs had already suggested that when the jobs were grouped into grades the picture might not alter; in fact, this proved to be the case.

The considerable overlap of rates, even between the lowest and highest grades, meant that the data did not offer as much assistance as had been hoped in suggesting appropriate money differentials between the grades. The national weekly minima of course provided a floor for the lowest grades of all, but they gave little guidance on the choice of rates for the remaining grades. These difficulties may be appreciated from the set of the grade frequency distributions shown on the left in the diagram. The typical shape of the job distributions was repeated for the grades, as appears from Table VI. As there were big differences in the range in job values between the lowest

TABLE VI

Aspects of the Wage Distribution for Daywagemen Elsewhere Underground

Grade	*Percentage of men earning less than 2s. above the minimum*	*Range*
I	27.5	25/10 to 69/–
II	67.7	25/10 to 43/3
III	70.7	25/10 to 42/6
IV	76.7	25/10 to 60/10
V	83.6	25/10 to 48/11

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[1] 'In modern large scale industry, all wage rates are administered wage rates; . . . the influence of economic forces is mediated through administrative decisions rather than expressed directly in the market place.' (Reynolds and Taft, *The Evolution of Wage Structure*, p. 1).

and highest grades, these figures were surprising. Although there was some evidence of a slight shift upwards in the means of the distributions for successive grades, there were no clear-cut differentials. Overlapping was so extensive as to render differences between grade averages, medians or modes of little assistance for selecting inter-grade differentials. The new grade structure therefore required radical changes in wage rates to remove anomalies and inconsistencies. Judged by the framework of the grade structure, the prevailing pattern of wage rates was haphazard and irrational. What was evident nationally extended even to individual collieries. The wage system for timeworkers in coal-mining seemed to be based more on the principle of the ' rate for the man ' than on that of the ' rate for the job '. Any relationship between the job or group of jobs of the same ' value ' and the rate appeared to be largely accidental, and in so far as it existed, appeared to be specific to particular collieries; even there, it was obscured more often than not by varying rates paid for identical jobs. The facts seemed to support the description of the British wage and salary structure which appeared not very long ago in *The Economist* as 'that cobwebbed and rickety tangle, almost without rhyme or reason. . .'.

Designing new structures

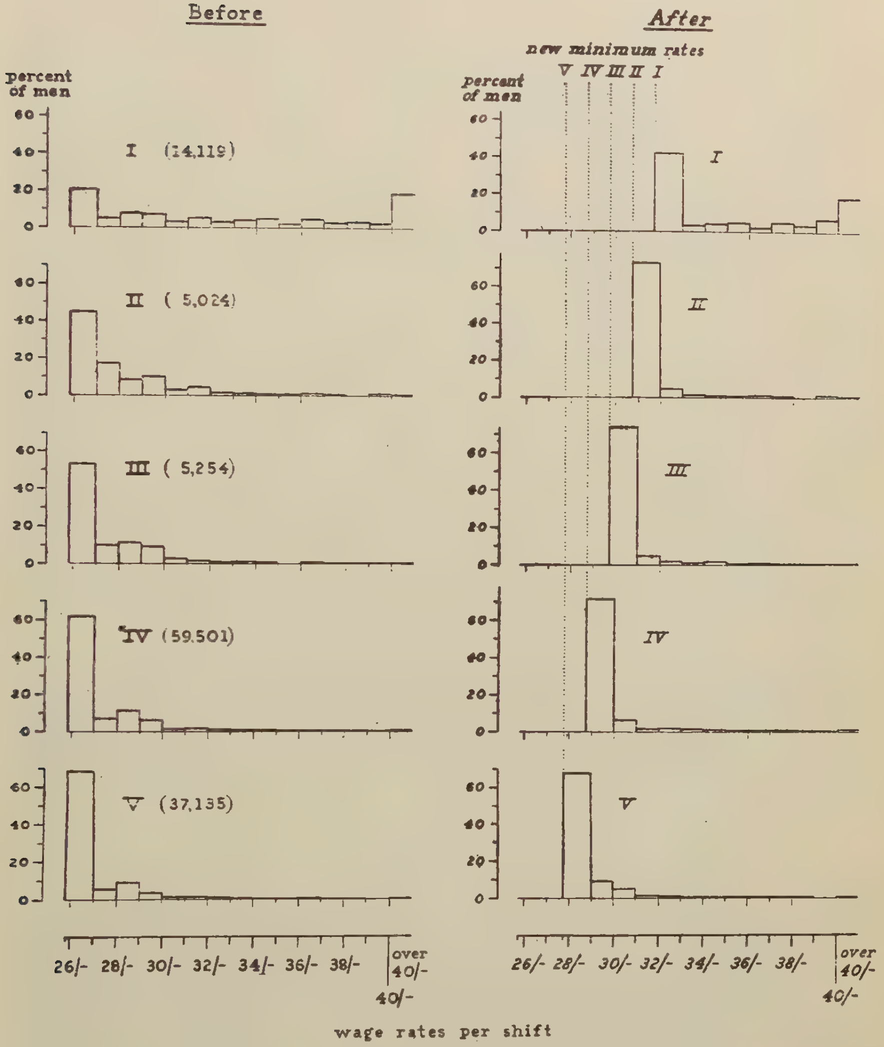
From the evidence provided by data from the ' trial assimilation ' it was now quite clear that there was very little in the existing scheme of things which could be saved and used again. Apart from the national weekly minima as a floor for the new wage structure (and the nationally prescribed rates of craftsmen and winding enginemen) there was little in the way of scaffolding. It would have been easier if the old structure could have been abolished outright. Its very existence imposed restrictions on the design of a new structure: its irrationality meant that even small changes would be costly and would disturb or destroy or, possibly, invert many long-standing differentials in wage rates. This could easily generate widespread dissatisfaction and cause industrial unrest, even though the changes were designed to produce fairer results and to remove admittedly unjustifiable differences.

It is one of the paradoxes of industrial relations that the application of principles agreed by employers and Trade Unions to be desirable may produce quite surprising and often unpredictable reactions when put into practice. One of the most difficult practical problems facing anyone who attempts to introduce substantial changes in a wage structure is how to institute such changes without generating industrial unrest, while keeping the cost within reasonable bounds. An essential of any new pattern is that it must be acceptable to the largest possible number of men. Whatever devotion to abstract principles may be displayed by workmen, whatever logic may be displayed in the pursuit of agreed objectives, the acid test is what happens to the pay packet. To be acceptable, changes in wage rates must be one-way —upwards. Crude and sudden reductions, even of anomalously high wage rates, were therefore ruled out. In short, redistributing the *existing* wage

The Revision of the Wage Structure

Daywagemen elsewhere underground

(The figures in brackets denote the numbers
of men in the new grades I to V.)



bill was impracticable; reliance would therefore have to be placed on a policy of differential wage *increases* designed to produce the desired result in the long run.

One obvious way of making a new structure acceptable was to ensure that as many as possible benefited from the changes. If this could be managed, the impact of some men getting more than others would be lessened. At the same time it was imperative that a new and properly articulated structure should contain clear-cut differentials between the grades. The construction of a pattern to meet these requirements at an economic cost was no easy matter. The problem, in essence, was to design a wage structure for 13 grades (plus an extra three standard rates for craftsmen working underground), by distributing a given aggregate increase in such a way as to award the maximum number of men some increase, while establishing differentials between the rates for the new grades. A detailed account of the way in which this problem was solved will not be given here. Mathematical techniques, and an extensive use of graphical methods, made it possible to devise a large number of possible patterns, each possessing certain merits and demerits. Before the final choice was made, over a hundred different patterns had been devised, and an evaluation made of their characteristics in terms of cost, coverage, distribution of wage increases by grades, and different impact on the various wage districts. It was obviously important that some degree of balance should be achieved between the wage districts and that the increases should cover as many people as possible. High national coverage could easily mean low coverage in particular districts. Every attempt was therefore made to predict the likely impact of a particular ' pattern ', not only on specific grades but on the several wage districts and even on individual collieries. Modifications were then introduced to counter any serious deficiencies in the pattern.

The volume of work entailed in these analyses was formidable, and most of it had to be crammed into a few months. In retrospect, it may be said that the care lavished on this work paid off handsomely, because eventually the new structure was introduced with surprisingly little friction considering the widespread changes which it entailed.

III. TOWARDS THE NEW WAGE STRUCTURE

A hypothetical structure

It is not possible to discuss within the limits of an article all the factors and arguments taken into account before the final structure was agreed with the National Union of Mineworkers. But a few illustrations may illuminate the gaps between the generalities of this account and the realities of the new structure. Speedy methods of determining the main characteristics of any desired pattern were essential, and ' ready reckoners ' were constructed for this purpose. These special tables proved of inestimable value both prior to and during negotiations in enabling quick and precise answers to be given to the majority of the ' questions arising '—particularly those concerned with the estimated cost of proposed changes.

The first illustration (Table VII) shows how the proposed grade structure looked in terms of the old wage rates for surface workers and for workers elsewhere underground, including craftsmen.

Table VII

National Profile Based on Trial Assimilation Data
(old wage rates, May 1954)

Category	*Grade*	*Lowest rate*	*Highest rate*	*Lower quartile*	*Upper quartile*	*Mean*	*Median*
Elsewhere underground:	I ...	25/10	69/9	27/5	37/11	33/6	31/5
	II ...	25/10	43/3	26/10	28/10	27/11	26/11
	III ...	25/10	42/6	26/6	28/5	27/7	26/10
	IV ...	25/10	60/10	25/10	27/6	27/2	25/10
	V ...	25/10	48/11	25/10	26/10	26/8	25/10
Surface:	IA ...	29/2	36/9	29/2	30/4	29/10	29/2
	I ...	22/6	32/4	23/6	24/8	24/7	24/1
	II ...	22/6	36/3	23/1	24/7	24/2	23/7
	III ...	22/6	38/11	22/6	24/–	23/8	23/1
	IV ...	22/6	39/2	22/6	22/9	22/11	22/6
Craftsmen:	I plus ...						
e.u/g.	...	27/6	36/11	31/–	33/9	30/10	33/9
surface	...	25/2	31/1	27/8	31/–	28/8	27/8
	I						
e.u/g.	...	25/10	47/3	31/–	31/–	30/7	31/–
surface	...	27/6	43/1	27/8	27/8	27/5	27/8
	II						
e.u/g.		25/10	46/5	27/4	29/10	28/11	28/6
surface	...	22/6	39/2	23/6	26/10	25/3	25/2

The second illustration (Table VIII) shows what could be done to the pattern shown above by spending about £7 millions per year[1] in accordance with the principles described in the preceding section. A strategy based on such principles called for substantial inter-grade differentials coupled with maximum coverage (*i.e.* as many men as possible getting increases). These are to some extent conflicting objectives, requiring highly specific and controlled changes in wage rates.[2]

In this particular pattern, the inter-grade differentials are at least 1*s.* per shift, and in some cases more. For example, a differential of 2/6*d.* occurs between the craftsmen's grades, and one of 3/8*d.* is introduced between surface grades IA and I. Also, between surface and underground grades containing similar occupations there is a uniform ' underground differential ' of 3/4*d.* Considerations of cost restrict the size of the differentials which it is possible to introduce in one step. However, even these changes bring about improved articulation. In this particular pattern, the mechanism selected is to set limits on increases for each grade. Both the limits and the amount of the flat-rate increases may be varied, in this case 1*s*. being chosen for the latter. In addition, the general level of the lowest wage rates (*i.e.* those for the bottom grades) is raised by 1*s*. per shift above the prevailing national weekly minima (or national rates in the case of craftsmen and winding enginemen). In summary form, the specification may be given as:—

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[1] Only part of the structure is illustrated here. The complete structure (including persons under 21) would on the same basis, require an expenditure of £8.5 millions.

[2] A full description of the procedure for introducing structures of this kind is given in the Joint Agreed Notes printed in the *Memorandum of Agreements, etc.*, for 1955.

(1) introduce a general flat-rate increase of 1*s*. subject to specified grade limits;
(2) raise lowest standard grade rates to 1*s*. above the prevailing national weekly minima (in the case of craftsmen and winding enginemen, above their national rates);
(3) introduce inter-grade differentials of at least 1*s*. (for craftsmen, 2/6*d*.).

TABLE VIII

A Hypothetical Wage Structure

Category	*Grade*	*Proposed standard rate*	*Limit*	*Percentage of men in each grade receiving some increase*	*Cost per year (£m).*
Elsewhere underground:	I ...	30/10	39/6	67.7	0.80
	II ...	29/10	36/–	90.8	0.25
	III ...	28/10	33/–	93.4	0.20
	IV ...	27/10	31/–	94.4	1.75
	V ...	26/10	29/–	93.6	0.62
		Total		88.2	3.62
Surface:	IA ...	30/2	34/–	98.7	0.06
	I ...	26/6	30/6	98.6	0.19
	II ...	25/6	29/–	98.9	0.54
	III ...	24/6	27/6	96.8	0.56
	IV ...	23/6	26/6	97.8	0.72
		Total		97.8	2.07
Craftsmen:	I plus				
e.u/g.	...	34/6	—	92.8	0.01
surface	...	31/2	—		
	I				
e.u/g		32/–	—	94.4	0.46
surface	...	28/8	—		0.45
	II ...				
e.u/g.	...	29/6	—	69.0	0.12
surface	...	26/2	—		0.10
		Total		89.1	1.14
		Grand total		*91.2*	*6.83*[1]

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[1] To arrive at the full cost, certain additions (covering holiday pay, guaranteed wages, minimum wages of pieceworkers, etc.) would have to be made to this figure.

Although the average increase is about 1*s*., individual workmen receive varying increases as the wage structure is pulled into its new shape. A more detailed analysis of the increases conferred by such a structure would give the picture shown in Table IX.

TABLE IX

Distribution of Hypothetical Increases based on Table VIII

Amount of increase (per shift)	*Per cent of men*
Nil	8.8
Less than 6*d.*	1.8
6*d.* and less than 1*s.*	2.3
1*s.* and less than 1/6*d.*	56.4
1/6*d.* and less than 2*s.*	2.8
2*s.* and less than 2/6*d.*	18.3
2/6*d.* and over	9.6
	100.0

The incidence of the wage increases produced by this pattern was also found to vary as between individual collieries. This was true of collieries within a given wage district as well, of course, as between collieries in different wage districts and divisions. However, except in one or two divisions (notably the East Midlands Division, where the proportion of men getting wage increases would have been substantially lower than elsewhere), the unevenness was, generally speaking, tolerable. The remedy lay in improving coverage in the wage districts of the East Midlands Division, and this was done by raising the national grade limits. A study of the grade frequency distributions of wage rates in districts where coverage was poor enabled grade limits to be chosen in a manner which reconciled maximum coverage with minimum cost. Similar analyses were carried out with the object of removing some of the objectionable features of the many other wage patterns tried out.

Finally, an attempt was made to measure the effect of the proposed changes on wage relativities in particular collieries. This was done by calculating, for each of these collieries, Spearman's rank correlation coefficient for the wages of workers in particular jobs before and after the introduction of the proposed wage structure. It was reassuring to find that where there was considerable disturbance this was compensated for by good coverage—*i.e.*, most workmen benefited to some extent.

The actual wage structure

The above wage pattern was not the final version, although it illustrates the experimental nature of the work involved. The structure actually put into operation in 1955 is shown in Table X.

The inclusive cost of these changes (including changes on similar lines for women and persons under 21[1]) was £14 millions a year. Immediately after the structure was introduced, it was calculated that about 70 per cent of all timeworkers would be on standard rates, 25 per cent on 'personal rates' (*i.e.* between the standard rates and the limits), while 5 per cent would 'mark time' (their rates being already above the limits).

Before the new structure could operate, all the jobs done by manual workers had to be sorted into the appropriate national occupations and graded.

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[1] See Appendix.

TABLE X

The New Wage Structure of April 1955

Category	*Grade*	*Standard rate*	*Limit*
Underground:	I	31/9	39/6
	II	30/9	36/–
	III	29/9	34/–
	IV	28/9	32/–
	V	27/9	30/–
Surface:[1]	IA	31/1	34/–
	I	27/5	31/–
	II	26/5	30/–
	III	25/5	28/6
	IV	24/5	27/6
Craftsmen:	I plus		
u/g.		35/5	—
surface		32/1	—
	I		
u/g.		32/11	—
surface		29/7	—
	II		
u/g.		30/5	—
surface		27/1	—

[1] For women (working on the surface in Grades I–IV) the standard rate was 4/11*d.* lower than the corresponding rate for men; the limit was 7/6*d.* lower in I and II, and 7*s.* lower in III and IV.

This process of *real* assimilation required the utmost care, since the wage rate and the amount of increase due would depend on the grading. Local officials of the Board and the N.U.M. jointly examined each man's job and placed it in the proper national occupation. Special appeal machinery was set up to deal swiftly with doubtful or disputed cases, which were referred for a final decision to a national committee composed of representatives of the Board and the N.U.M. The main part of this operation was accomplished in less than three weeks, and only about 6,000 disputed cases arose—a small proportion of the total number of workers involved.

Stages in implementing the structure

There remained the final step of applying the new wage rates to each man. The mechanics of this process are described in detail, as are the mechanics of the Joint Assimilation, in the ' Joint Agreed Notes on the Revision of the Wages Structure Agreement '.[1] Certain operations had to be carried out in strict sequence. Firstly, all occupations had had to be graded: secondly, wage rates had to be consolidated: thirdly, consolidated wage rates had to be raised by 1*s.* per shift subject to the grade limits. Thus, workmen whose wage rates were 1*s.* or more below the appropriate grade limit got 1*s.*; those only 11*d.* below got 11*d.*; those 6*d.* below, 6*d*; and so on—the increases tapering off to nothing for those whose wage rates were equal to or higher than the limit. The latter group were called the ' mark-time ' men. Finally, all wage rates still below the appropriate standard grade rate were lifted up to the standard rate.

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[1] These are to be found in the *Memorandum of Wage Agreements, etc.*, produced annually by the National Coal Board.

Before the change, the national weekly minimum for all underground workers irrespective of grade or occupation was 25/10*d.* per shift. This was lifted by 1/11*d.* per shift to form the lowest standard rate for underground workers in the revised structure. Then, inter-grade differentials of 1*s.* were introduced. Thus, the lowest rate in Grade I was raised by 5/11*d.*, the lowest rate in Grade II by 4/11*d.*, and so on—diminishing to 1/11*d.* for the bottom grade, Grade V.

Since April 1955, it is incorrect to designate the lowest rate for any of the grades as a ' minimum ' rate.[1] In the terminology of the revised structure they are ' standard ' rates. There is a significant difference between a ' standard ' and a ' minimum ' rate. The latter implies that there is freedom to pay a workman any wage rate so long as it is not less than the minimum. In coal-mining this is no longer so: the standard rate is the only recognised rate for the job and grade. Rates above standard are restricted to those men who were paid such rates when the new structure was introduced: and these ' personal ' rates are countenanced only because reductions in rates were ruled out. For two reasons, however, these rates should disappear in due course: firstly because a man continues to be entitled to his personal rate only so long as he remains in his existing grade at the same colliery; and secondly because personal rates would tend to be overtaken by a succession of general wage increases.

Similarly, the ' limits' associated with each grade are not maximum wage levels. Their function is to limit the amounts which successive general increases may bring. The gap between standard rates and limits in each grade should gradually close until in due course the two merge: until then, the ' limits ' should act as barriers at which personal rates will collect.

IV. General Observations

The new model introduced in April 1955 may not be ideal, but the structure is adaptable and will no doubt be improved with time. Considerations of cost prohibited the immediate introduction of more substantial differentials between standard rates. Even slight changes in differentials involved heavy cost because of the concentration of men at the lower end of the wage spectrum of each grade. The immediate introduction of more substantial differentials would also have meant greater disturbance of wage relativities, possibly to the point of explosion. In an industry previously accustomed to more or less uniform flat-rate increases, substantial differential wage increases on this scale were a novelty, and not without risk. There was, therefore, much to be said for gradualism.

There can be little doubt about the superiority of the new wage structure, even in its first stage of metamorphosis, over the old jumble of wage rates. Adjustment of the old structure to meet specific objectives was extremely difficult, because of ignorance about the scope of particular classes or occupations. Consequently any attempt to do so was a hit-or-miss affair. Even the most gingerly attempts at exercising differential wage policy in these conditions were frustrated almost before they could be started. It is no accident that for many years in coal-mining flat-rate increases were the rule, with occasional very minor deviations.[1] The necessary framework for doing anything else was simply not available, for the existing wage structure was unsatisfactory in itself.[2] A wage structure besprinkled with anomalous and apparently purposeless wage rates may also be felt to be unfair, in that it does not observe the Trade Union principle of ' the rate for the job '. It is a matter of common experience that anomalous wage rates sometimes produce an undercurrent of widespread discontent, which may on occasion reach explosion point. It is moreover not uncommon for such irrational differentials to generate inflationary tendencies by a kind of industrial magnetism: the pressures are invariably for upward revision of all wage rates towards the anomalously high rate. When such pressures are being experienced in their tens or hundreds at varying points in an industry, each aiming at getting wage rates to the level of its own ' anomaly ', any tinkering with one part is liable to set up an uncontrollable chain reaction in the whole. This may create widespread disturbance, and yet leave things relatively much as they were before the first change was triggered off. In such situations, it is impossible to know in advance where a seemingly trivial and possibly justifiable change will end, and the process may be as expensive as it is unsatisfactory.

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[1] The standard rate for timeworkers does, however, act as a ' minimum ' rate for pieceworkers: for example, if their earnings on piecework for a normal week's work falls below a figure equal to six times the appropriate standard grade shift rate.

The reform of the system on the lines taken was thus an essential preliminary to applying, in a more intelligent and rational manner, sums of money which would otherwise be devoted to flat-rate increases and so to the perpetuation of a wage structure out of tune with the needs of a nationalised industry. An anachronistic wage structure, whatever its justification under past conditions, may through inertia and resistance to change become a drag on an industry. Some persons may think that a wage structure is flexible[3] only so long as it is chaotic, anomalous, incomprehensible when viewed as a whole, impossible to administer, and atomistically determined by unilateral managerial decisions. As a tool for carrying out a jointly agreed wage policy, the new structure would appear to be capable of more real flexibility and of incomparably greater precision for translating decisions into action; and there is now little fear of setting out to do something only to find that the result turns out quite different in practice. *Whether or not its potentialities are realised depends not so much on the instrument itself as on the users.* Neither the current grading of occupations nor the standard rates applied to them need remain immutable for all time. New jobs may be added and other jobs renovated to keep abreast of technical developments, whilst others will become obsolete. Similarly, the newly established wage differentials may well need altering. In our view, the revised structure is very much more adaptable than the old to changing circumstances and changing needs.

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[1] Differential wage increases had previously been tried out with limited classes only; *e.g.* between craftsmen and winding enginemen, or between broad categories such as surface and underground workers. The industry was accustomed to wage ceilings, but even these had sometimes to be removed in practice.

[2] On more than one occasion, the National Reference Tribunal of the Coal Mining Industry felt this difficulty and recommended an overhaul of the wage structure—for example, in the First Porter Award (January 1944). Again, the Twenty Fourth Award (October 1952) stated that ' . . . a more National wage structure is a matter of vital importance. The Tribunal have always been conscious of the desirability of a reconstruction of the wage system and in the past have recommended its consideration and conclusion '.

[3] Flexibility in the geographical sense, *i.e.* the freedom to change rates to suit local economic conditions, had been virtually non-existent in coal-mining since 1944.

CONCLUSION

In this article we have attempted to describe some of the main problems encountered in introducing a new wage structure into coal-mining, and to sketch some of the background. Much has been written about the technical reconstruction plans of the industry, relatively little about the reconstruction work done in the field of industrial relations. Our hope is that this article will to some extent redress the balance. So far as our knowledge goes, nothing on this scale has been done in this country before. That such a drastic change was introduced with very little disturbance and unrest in an industry usually regarded as among the most militant is in part a tribute to careful planning and meticulous execution, but much more to the ready acceptance by the workmen of its principles. Any number of structures may be devised, but however ingenious they may be, however cunningly contrived, they can only become effective in practice with the consent of the men and their Trade Union representatives.[1] A structure may possess great merits—economic or otherwise—yet fail to work because it is simply not acceptable. Co-operation is no less necessary for the long-term success of the structure in securing a greater measure of distributive justice within the coal-mining industry. *The new wage structure will work effectively only so long as there is a joint determination to abide by the basic principles of the structure, which have been codified in the National Agreement.* In other words, it requires the continual exercise of discipline from management and men. It will take time to accustom hundreds of thousands to the way in which these principles must work in practice. To wipe out at one stroke a great number of local customs and practices governing allowances and other special payments by substituting a simple rule which permits one rate and one rate only to be paid for a given job wherever performed, may not be a novel idea; but its application in coal-mining to timeworkers was certainly something unusual. Consequently, it is understandable that isolated attempts may be made to continue the old practices. Local pressures to depart from the basic principles and rules in various ways are usually greatest at the very beginning, before the new ideas have had time to percolate. This is the time when the strictest surveillance must be kept on the working of the structure, and when steadfastness above all else is required on all sides and at all levels. This is made very much easier when jointly agreed principles can be enlisted to thwart deviations or to overcome the temptation to tinker with the structure.

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[1] The task of reorganising an industry's wage structure is, of course, made easier where there is but one employer. The task is also easier when the Trade Union concerned has a relatively simple structure and when negotiations are carried on through a well developed system of collective bargaining.

Appendix

Persons under 21

In coal-mining, a rate-for-age system applies to timeworkers who are under 21 years of age, there being two scales—one for surface and one for underground workers. Except that the framework for adults is based on jobs and that for persons under 21 is based on age and place of work, the principles followed in revising the wage structure for juveniles are virtually identical. Although national minimum rates for age had been introduced in 1943, the superstructure of district and pit rates for age remained almost unchanged. It was felt that the occasion was opportune for evening out all districts and pit rates, and for paying all youths on timework the same rate for age (apart from the underground differential).

National standard rates for age were therefore introduced, together with limits on increases. By April 1960, even if no wage increases supervene, all personal rates for juveniles should have disappeared. The pattern actually introduced in April 1955 was as follows:—

New Wage Structure for Juveniles

Age	*Underground*		*Surface*		
			Males		*Females*
	standard rate	*limit*	*standard rate*	*limit*	*standard rate (no limits required)*
15	15/3	16/9	13/–	14/–	12/8
15½	15/9	17/3	13/7	14/7	13/3
16	16/3	17/9	14/1	15/1	13/9
16½	16/10	18/4	14/6	15/6	13/10
17	17/6	19/–	15/1	16/1	14/1
17½	18/4	19/10	15/6	16/6	14/6
18	20/11	23/5	18/2	20/3	16/8
19	22/3	25/6	19/2	21/3	17/8
20	23/6	27/–	20/6	22/6	18/8

ON MEASURING BUSINESS CONCENTRATION[1]

By P. E. HART

1. INTRODUCTION

It is possible to distinguish two approaches to the analysis of business concentration. First, there is the *statistical* approach, stemming from the work of Pareto, Lorenz, Gini and Gibrat, which relates concentration to the inequality of size distributions. A study prepared at the National Institute of Economic and Social Research[2] extended these methods and applied them to all manufacturing firms quoted on the London Stock Exchange, and similar techniques will be used in this paper.

Secondly, there is a more recent approach beginning with the pioneer article by Means[3] in which the largest 200 non-financial corporations by assets were identified and expressed as a proportion of all non-financial corporate assets. This method may be termed the *enumerative* approach: it is distinguished from the statistical approach because it does not involve the use of statistical summary measures, the object being to enumerate rather than to summarise. Means continued this analysis in the report on the *Structure of the American Economy*[4] where the enumerative approach was applied to individual manufacturing industries, by calculating the proportion of each industry's product controlled by the largest four firms. Such proportions are termed absolute concentration ratios and can be calculated for any number of firms.

In the United Kingdom, the enumerative approach was adopted by Leak and Maizels[5] in 1945 and concentration ratios were calculated for the largest three firms in each industry in 1935. The National Institute has repeated this exercise in 1951 so that it will be possible to calculate changes in concentration in certain industries between 1935 and 1951.

The purpose of this paper is to expose the links between the two approaches by applying both of them to one industry: by comparing their results it should be possible to learn which approach to adopt to solve any particular problem. Needless to say, these analyses are merely preliminary in the sense that a detailed descriptive study is generally necessary before reaching firm conclusions on the extent of monopoly or competition in the industry under investigation. This is certainly true of the brewing industry studied in this paper. The reason for selecting this industry is mainly technical ; quoted brewing companies can conveniently be identified over a long period of time from the Stock Exchange category, ' Breweries and Distilleries '. In addition, it has a large number of firms and thus makes a statistical approach worthwhile.

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[1] I am very grateful to Mr. W. A. B. Hopkin, Dr. I. M. D. Little and Dr. S. J. Prais for their criticisms of an earlier version of this paper and for their general guidance, but they are not responsible for any of its shortcomings. I should also like to thank Miss B.M. Swift for assistance in the compilation of the tables. This study forms part of the National Institute's research project on business concentration and has been assisted by a grant under the Conditional Aid Scheme for the use of counterpart funds derived from United States economic aid.

[2] P. E. Hart and S. J. Prais, 'The Analysis of Business Concentration: a Statistical Approach', *Journal of the Royal Statistical Society*, Series A, Part 2, Vol. 119 (1956).

[3] G. C. Means, 'The Large Corporation in American Economic Life'. *American Economic Review*, Vol. XXI (March 1931).

[4] G. C. Means, *The Structure of the American Economy: A Report prepared by the Industrial Section of the National Resources Committee*, Part I. (Washington: 1939) Appendix 8, page 264.

[5] H. Leak and A. Maizels, 'The Structure of British Industry', *Journal of the Royal Statistical Society*, Vol. CVIII, Part II, 1945.

2. THE ENUMERATIVE APPROACH

Measures of size

At present, financial measures of size are available only for those business units quoted on the Stock Exchange, so the comparison of concentration measures has to be confined to the quoted portion of the brewing industry. For recent years it is possible to measure size by net tangible assets,[1] and over a longer period it is possible to measure a company's size by the market value of its capital. In Table I the ten largest business units engaged in brewing and malting in 1935 and 1951 are ranked by their net tangible assets. Of course, the firms listed can only be taken to be the largest on the assumption that there are no unquoted business units which are larger. In this case, such an assumption is reasonable.

TABLE I

Net Tangible Assets of Quoted Business Units in the Brewing Trade 1935 and 1951

1935			1951		
Business unit	*N.T.A.*	*Cum. %*	*Business unit*	*N.T.A.*	*Cum. %*
	£mn.			£mn.	
1. Watney, Combe, Reid	18.0	5.8	1. Watney, Combe, Reid	20.6	4.9
2. Charringtons ...	15.6	10.8	2. Ind Coope ...	20.0	9.6
3. Bass, Ratcliffe ...	15.0	15.6	3. Guinness	19.1	14.1
4. Walker and Cain ...	14.7	20.4	4. Charringtons ...	15.5	17.8
5. Ind Coope ...	13.3	24.7	5. Bass, Ratcliffe ...	15.1	21.3
6. Guinness	13.0	28.9	6. Whitbreads ...	13.0	24.4
7. Taylor Walker ...	10.3	32.2	7. Walker and Cain	12.7	27.4
8. Barclay Perkins ...	10.0	35.4	8. Truman ...	12.3	30.3
9. Mitchells & Butlers	8.2	38.0	9. Scottish Brewers	12.2	33.2
10. Ansells	7.1	40.3	10. Courage ...	12.1	36.0
	125.3	40.3		152.7	36.0
11–165	185.4	59.7	11–142...	270.9	64.0
	310.7	100.0		423.6	100.0

Source: Appendix.

It can be seen that each of the top six in 1935 appeared within the top seven in 1951, though their rank order was slightly different. As this approach is extended—by considering firms below rank 10 or by considering more than two years—it becomes more difficult to see the extent to which firms overtake each other in the competitive struggle.

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[1] Net tangible assets equal total assets (net of depreciation) *less* current and deferred liabilities and provisions.

This is made apparent by Table II which gives the largest ten business units, measured by market valuation, in the five years 1896, 1907, 1924, 1939 and 1950. An inspection of this table shows that Guinness was the largest firm in each year, but that the rank order of the next nine firms was different in each year. But it is impracticable to compare the size mobility of the firms between different periods simply by inspecting the tables; a summary measure of size mobility would be required to do this.

A comparison of Tables I and II shows that the largest six firms ranked by assets in 1951 all appeared among the largest ten ranked by market valuation in 1950. These in turn may be compared with rankings by income in 1954 and 1955 which,[1] while showing that Guinness was consistently the largest, suggest that rank by income differs slightly from rank by market valuation and by assets. Though there is a high correlation between market valuation, assets, and income, there will of course still be small differences in the ranking of firms according to the measure of size that is used.

It would be desirable to measure the size of business units by more ' solid ' measures such as employment or output, for it would then be possible to construct concentration ratios which could be compared with those prepared by Leak and Maizels for 1935 and by the National Institute for 1951, based on the top three units. But the observed variations in rank according to the size measure used suggests that it is possible for measures by employment, by gross output and by net output to result in different rankings and even in different firms occupying the top three places.

In practice, the concentration ratios calculated from the Census of Production are based on employment, so that the concentration ratio in terms of gross output, for example, relates to the gross output of the largest three firms in rank order of employment, and does not measure the largest three firms in rank order of gross output. Such concentration ratios, relating to the whole of the brewing industry, are given in Table III. They may be compared with those relating to that part of the brewing industry lying within the quoted sector. In 1951 business units quoted on the Stock Exchange controlled 72 per cent of the employment and 78 per cent of the gross output of the Brewing and Distilling trades, which suggests that the concentration ratios for 1951 in rows 4 and 5 should be reduced to about 11 and 19 respectively.[2] The observed ratio in terms of market valuation

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[1] The National Institute of Economic and Social Research, *A Classified List of Large Companies engaged in British Industry* (London: December 1955), p. 24. The income: assets ratio of Guinness is more than twice that of other giant breweries, which explains why it has the largest market valuation.

[2] Separate figures for the Brewing trade are not available and the use of the combined figures for Brewing and Distilling is thought to underestimate the degree of concentration in the Brewing trade.

TABLE II

£m *Market Valuation of Quoted Business Units in the Brewing Trade, 1896–1950*

	1896 M.V.	*Cum. %*		*1907 M.V.*	*Cum. %*		*1924 M.V.*	*Cum. %*
1. Guinness	14.0	21.3	Guinness	13.8	16.2	Guinness	31.5	25.0
2. Allsopp	4.6	28.2	Watney	7.3	24.8	Watney	8.6	31.8
3. Bass, Ratcliffe ...	3.7	33.9	Allsopp	3.5	28.9	Bass, Ratcliffe	6.1	36.8
4. Walker, Warrington	2.5	37.7	Bass, Ratcliffe	3.3	32.8	Walker and Cain ...	6.1	41.6
5. City of London ...	2.4	41.4	Whitbread	2.7	35.9	Mitchells and Butlers ...	4.8	45.4
6. Threfalls	2.4	45.0	Barclay, Perkins ...	2.4	38.7	Threfalls	3.9	48.5
7. Combe	2.3	48.6	Threfalls	2.2	41.3	Courage	2.5	50.5
8. Watney	2.3	52.1	Mann, Crossman	2.1	43.8	City of London	2.1	52.2
9. Whitbread	2.0	55.1	Mitchells and Butlers ...	1.9	46.1	Ind Coope	2.1	53.9
10. Reids	1.7	57.7	Cannon	1.9	48.3	Allsopp	1.8	55.3
	37.9			41.1			69.7	
11–81	27.8	42.3	11–132	43.9	51.7	11–141	56.4	44.7
	65.8	100.0		84.9	100.0		126.1	100.0

	1939 M.V.	*Cum. %*		*1950 M.V.*	*Cum. %*
Guinness	40.1	12.2	Guinness	51.0	13.0
Bass, Ratcliffe	21.1	18.7	Ind Coope	23.1	18.9
Watney, Combe, Reid	19.4	24.6	Bass, Ratcliffe	22.0	24.5
Ind Coope	19.3	30.4	Watney	20.1	29.6
Charringtons	14.7	34.9	Mitchells and Butlers	13.8	33.1
Mitchells and Butlers	14.3	39.3	Charringtons	13.0	36.4
Walker and Cain	11.3	42.7	Walker and Cain	12.5	39.6
Ansells	9.9	45.7	Ansells	10.7	42.3
Barclay Perkins...	9.6	48.6	Taylor Walker	10.0	44.8
Taylor Walker	9.2	51.5	Whitbread	9.0	47.1
	169.0			185.2	
11–166	159.4	48.5	11–160	207.7	52.9
	328.4	100.0		393.0	100.0

is thus seen to be higher than the others, while that in terms of net assets equals the lowest ratio, that in terms of net output.

TABLE III

Concentration Ratios for the Brewing Trade Trade 1935–1951

3 largest business units as per cent of	*1935*	*1951*
1 Employment of all Units	16	13
2 Gross Output of all Units	15	14
3 Net Output of all Units	18	11
4 Net Tangible Assets of Quoted Units	18	14 (11)
5 Market Valuation of Quoted Units	25*	24†(19)

* Market Valuation for 1939. † Market Valuation for 1950.

Each calculation suggests that the degree of concentration decreased slightly over the period 1935–1951. Owing to the absence of information, it is not possible to conduct any further analysis of the concentration ratios in terms of employment, gross output and net output. But changes in those in terms of assets and market valuation can be explained. It is possible to show whether the deconcentration is a freak result which would not apply if some number other than three were chosen for the number of firms to be measured in the concentration ratio. It is also possible to calculate the effects on concentration of amalgamations and of changes in the business population. Finally, it is possible to know whether the top three firms in 1951 were the same as in 1935—knowledge which is not available in the Census concentration ratios, but which may be important in judging the competitive behaviour within an industry.

Concentration curves

Cumulative percentage frequency distributions are shown in Figure 1 for the largest ten firms in descending order of size, measured by market valuation 1896–1950 and by net tangible assets 1935–1951. The height of any one of these ' concentration curves '[1] above any point on the horizontal axis measures the proportion of the quoted part of the industry accounted for by the largest x firms. Of course, students of concentration in the brewing industry would like to see such curves for the whole industry and not just the quoted part of it, or would like to know the proportion of the industry controlled by quoted companies at each of the dates in Figure 1. The absence of such information limits the relevance of these curves as guides to changes in concentration in this industry over the period 1896–1951. But it does not affect the present exercise, which is primarily an experiment in applying two techniques of measuring concentration to a given population of brewing firms: the comparative advantages, or disadvantages, of each technique would still apply to any study of concentration in the industry as a whole, if such a study were possible. Thus the present curves clearly show the disadvantages of analysing changes in concentration in terms of concentration ratios. Such ratios, which are equivalent to one ordinate of a concentration curve (generally $x = 3$ because this is the minimum value Census authorities are prepared to disclose), cannot provide an unequivocal answer to the question, 'Has concentration increased or decreased?' The concentration curves may cross so that $x = 2$ reflects a rise, and $x = 3$ reflects a fall in concentration.

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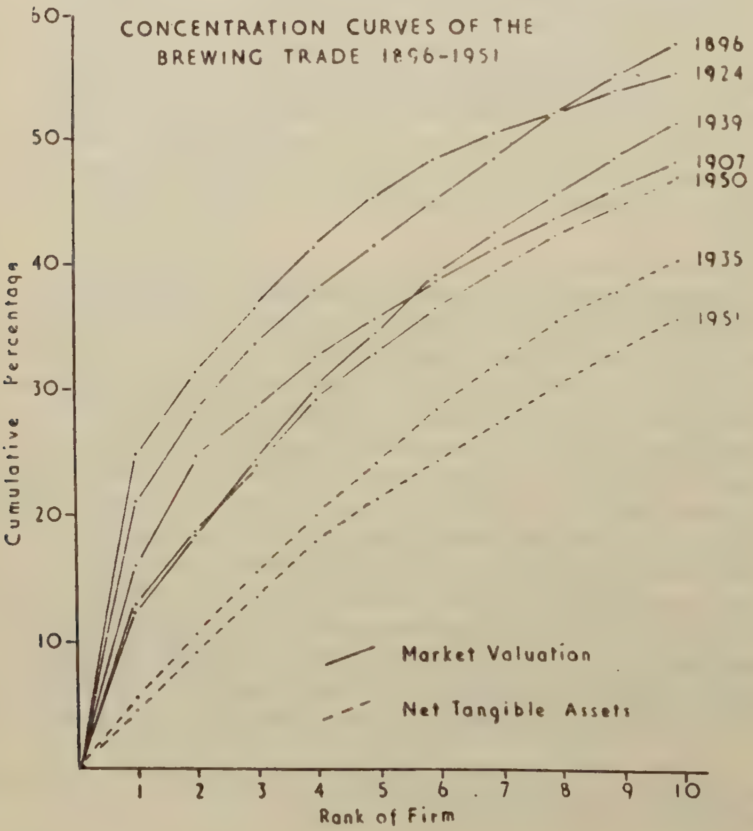
[1] *cf.* Federal Trade Commission, *The Concentration of Productive Facilities, 1947.* (Washington 1950) and G. Rosenbluth, ' Measures of Concentration ', in *Business Concentration and Price Policy*. (Princeton: 1955).

Such a conflict has not arisen in the 1935–1951 comparison in terms of net assets: all values up to $x = 10$ show a fall in concentration over the period. But the curves for 1939 and 1950, in terms of market valuation, show that concentration increased slightly for values $x<3$. Even more pronounced is the crossing of the curves for 1907 and 1939 showing that concentration decreased for $x<5$ and increased for $x>6$. Some average measure of concentration for each year seems to be indicated, and such a measure is given in the statistical approach of section 3.

Figure 1. *Concentration curves of the Brewing Trade*, 1896–1951



Changes in business population

A concentration ratio may change over time, even if the largest firms remain the same size, as the result of firms entering or leaving the industry. Since this study is concerned with only that part of the brewing trade which is controlled by breweries quoted on the Stock Exchange, the figures of new quotation ('births') and loss of quotations ('deaths') will have to be used to estimate the effects on concentration of changes in the business population. While the appearance of firms seeking more capital in the market, and the disappearance of old firms as the result of amalgamation or bankruptcy, may be a reflection of whether the industry as a whole is expanding or contracting, there is no strong evidence to support the present assumption that the effects on concentration of changes in the quoted population are similar to those of the true births and deaths within the whole of the brewing trade. On the other hand, there is no reason for not making this assumption.

The effects of the observed births and deaths on concentration in the quoted sector, as measured by the concentration ratio for $x = 3$, may be seen in Table IV. The difference between rows 1 and 2 is due to deaths, the difference between rows 3 and 4 is due to births, and the difference between rows 2 and 3 measures the change in concentration among surviving firms. The effects of births and deaths may be summarised by saying that they have influenced the level of concentration, but they do not appear to have influenced the change in that level. The change in concentration in any period seems to be governed by surviving firms. An exception to this generalisation occurs within the period 1896–1907, when the number of births[1] was relatively large and had a deconcentrating effect.

TABLE IV

The Effect of Births and Deaths on the Observed Concentration Ratios

	1896–1907	*1907–24*	*1924–39*	*1939–50**
1. Total 1st year	34	29	37	25
2. Survivors 1st year ...	37	32	41	25
3. Survivors 2nd year ...	37	42	31	25
4. Total 2nd year	29	37	27	25

* The comparison of concentration ratios between 1939 and 1950 is based on data relating to the combined Official and Supplementary Lists.

The effect of amalgamation

Having ruled out changes in business population as a determinant of the change in concentration within this sector of quoted brewing firms, we may consider the effect of amalgamations, which have so often been regarded as an important force making for increased concentration. It is possible to trace amalgamations between companies quoted on the Stock Exchange. It is also possible to include some liquidations where the assets have been acquired by another company.

[1] *Cf.* Tables IV and VI.

An inspection of the original data reveals that in the first period, 1896–1907, the amalgamation between Watney, Combe and Reid tended to increase the concentration ratio by some four or five points. But this tendency was swamped by the fact that the other members of the top three in 1907, Guinness and Allsopp, grew less rapidly than the remaining firms in the industry, so that the concentration ratio fell from 34 to 29 over the period 1896–1907. No other amalgamations involved a member of the top ten in this period, and any amalgamations involving the lower ranks could not have had any significant influence on the concentration ratios.

In the periods studied between 1907 and 1950 there were instances of amalgamations and acquisitions involving a member of the largest ten, but none of them had any significant influence on the concentration ratio. Generally, such amalgamations took the form of a giant company acquiring a relatively small company, and such an increase in size had little effect on the total size of the largest ten, and, therefore, little effect on the numerator of the concentration ratio for $x = 10$. There were no amalgamations comparable with the Watney merger: even the amalgamation between Ind Coope and Allsopp increased the concentration ratio for $x = 10$ by only 1 point, and did not affect the ratio for $x = 3$ at all.

The upshot is that amalgamations have had little effect on concentration in the sector of quoted brewing companies over the period 1896–1950. This conclusion can be safely applied to the whole of the brewing trade because amalgamations involving unquoted brewing firms are unlikely to have had any significant effect on concentration.

Size mobility

The results of the preceding two sections were obtained by enumerating the data and omitting or retaining births, deaths or amalgamations as required. The enumerative approach is continued in the present section on the size mobility of the leading firms. The reason for studying size mobility is that an industry is more likely to be competitive if there are considerable changes in rank order than if there are very few changes and the same firms are always the largest. It is the problem of business concentration in a dynamic context.

The fact that different measures of size yield different rank orders of business units, makes it difficult to say whether the largest three firms covered by the Census concentration ratio in 1935 were the same as those in 1951. From Table I it can be seen that when size is measured by market valuation, Guinness is the largest firm in both 1939 and 1950, Bass falls from second to third place over the period, and the other places in the top three are occupied by different firms in each year.

This impression of changing leadership is reinforced by an inspection of changes in the ranks of companies within the largest ten. But without some method of summarising changes in rank order, it is difficult to ascertain whether the degree of size mobility in 1935–51 was high or low, or whether size mobility in any period was different from that in other periods. However, the enumerative approach does suggest that this industry is competitive; first because concentration is low, secondly because the concentration ratio for $x = 3$ has decreased 1935–51, and thirdly because the leadership appears to have been continuously changing over time. A possible exception to the third generalisation is the position of Guinness, a producer of a highly specialised beer and one which is not engaged in tied house competition in this country.

3. The Statistical Approach

When the number of firms in an industry is large, a statistical approach is useful in summarising the changes in concentration. Of course, in many industries which interest all students of business concentration the number of firms is too small to make a statistical analysis worthwhile. But this is not the case in the brewing industry and the statistical approach can be applied to each of the problems of section 2, namely the rate of growth of large firms relatively to small firms, the effects of amalgamation on these rates of growth, the effects of the entrance and exit of firms, and the extent to which firms change rank order.

The Statistical Measure of Concentration

The statistical approach to business concentration is based on the assumption that the size distribution of firms tends towards lognormality. The formal tests of this assumption are given in the Appendix, but while these yield satisfactory results for the present purposes of graduating data, it does not necessarily follow that this is the most appropriate assumption to adopt. However, the arguments for and against making such an assumption[1] are outside the scope of this paper, which may be regarded as assessing the advantages and disadvantages of the statistical approach, *if the distributions were approximately lognormal.*

The statistical measure of concentration in this context is the variance of the logarithms of the sizes of firms. This is denoted by σ^2 and is a measure of relative dispersion. It is a parameter of the pure lognormal distribution and determines the Gini coefficient of concentration. Since the Gini coefficient is a summary measure of all the percentage concentration ratios, it follows that, providing the total number of firms is given, in a lognormal distribution any absolute concentration ratio can be calculated from the variance.[2]

These calculations have been made and the ‘ expected ’ concentration ratios, which may be compared with the concentration ratios observed for the same business population, are given in Table V. It can be seen that there are

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[1] This controversy has a long history. See J. Aitchison and J. A. C. Brown, *The Lognormal Distribution* (Cambridge: 1957), pp. 2–5 and 20–22.

[2] The technique of calculating concentration ratios from the variance of a lognormal distribution follows from J. Aitchison and J. A. C. Brown. On Criteria for Descriptions of Income Distribution, *Metroeconomica*, Vol. VI (Dec. 1954). The Prais slide-rule was used to calculate the expected ratios in Table V. *Cf.* Hart and Prais, *op. cit.* p. 160.

TABLE V

'Expected' and Observed Concentration Ratios for Quoted Brewing Companies 1897–1951.

	Top 3		*Top 10*	
	'Expected'	*Observed*	*'Expected'*	*Observed*
Market Valuation:				
1896	28	34	50	58
1907	21	29	40	48
1924	26	37	46	55
1939	32	27	55	56
1939	28	25	48	52
1950	28	25	50	47
Net Tangible Assets:				
1935	15	16	32	40
1951	18	14	36	36

generally differences between expected and observed ratios. The predictions are based on simple two-parameter lognormal distributions, and yet it is known that the observed distributions are truncated. But even if better fits were obtained by introducing a third parameter to allow for truncation, it is doubtful whether greater accuracy could be achieved in the prediction of concentration ratios based on the largest three firms. A glance at Table II shows that the observed concentration ratios are governed by the size of the largest firm, Guinness, and it is unlikely that a simple distribution could predict the size of one firm with any accuracy: yet the size of the largest firm is very important in the value of a concentration ratio. This means that in the calculation of the amount of the industry controlled by a few large firms at one moment of time, there is no substitute for the enumerative approach. This may well be a general rule, for it is unreasonable to expect a simple distribution to supply an exact fit to one or two observations in an extreme tail, even if it provides a satisfactory fit to most of the data.

However, this conclusion is subject to a major qualification when comparisons—spatial or temporal—of concentration are being explained. When comparing the same industry in different countries, it is important to know whether the differences in concentration are due to differences in the number of firms or to differences in inequality.[1] Thus the concentration ratio has to be supplemented by the knowledge of the number of firms and by some measure of inequality. If the size distributions are approximately lognormal, then the variance of the transformed distribution is an appropriate measure of inequality.[2]

Again, when comparing the concentration curves of the same industry at different points of time, it is sometimes necessary to have a summary measure, as argued in section 2, of the general trend of changes in concentration.

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[1] *Cf.* G. Rosenbluth, Industrial Concentration in Canada and the United States, *The Canadian Journal of Economics and Political Science*, Vol. XX (August 1954), p. 335.

[2] This is similar to comparing two points on two Lorenz curves, on the assumption that they do not cross, for the Lorenz curves of lognormal distributions are symmetrical. *Cf.* Rosenbluth, *op. cit.*, Table II, p. 341 and his conclusions on p. 335.

The variance of the logarithms of firms' sizes can also be used as a summary measure of concentration in this case. The statistical approach makes it possible to compare levels of concentration, and to answer interesting questions such as, ' Is the brewing industry becoming more concentrated? Is it less concentrated in the United Kingdom than in the United States? Is it less concentrated than other industries? ', with concentration defined as relative inequality. Answers based on concentration ratios are invariably arbitrary since they depend upon the number of giant firms chosen to be measured in the numerator of the ratio. That is, even in a comparative static analysis of business concentration, the enumerative approach is insufficient by itself, and has to be supplemented by the statistical approach for the purpose of making comparisons of concentration.

So far, the suitability of the variance of the logarithms as a measure of concentration has been assessed by its capability of providing the same answers to the same questions on concentration as those provided by a concentration ratio. But the variance of the logarithms is also useful because it answers questions on concentration which are slightly different from those which a concentration ratio is supposed to answer. The variance is a direct measure of the inequality of all firms' sizes and inequality must also be considered when measuring the scope for monopolistic or oligopolistic practices in an industry. Though this paper is not primarily concerned with the economics of imperfect competition, it is worth providing some illustration of the economic implications of inequality at this stage.

The existence of very small firms and giant firms side by side in the production of the same commodity suggests that the scope for price leadership is very high. That is, the giant firms set the output or price policies and the small firms follow them. Thus an increase in the variance of firms' sizes, as the result of an increase in the size of the large firms relatively to the small firms,[1] may result in an increased scope for the domination of the small firms by the large firms and, therefore, may reduce the scope for competition. Alternatively, the existence of a high degree of inequality may imply that the giant firms can afford to ignore the policies of the small firms, especially if the small firms' products are slightly different from that of the giant firms. Thus competition between large and small firms would be low, and an increase in the variance, as the result of the growth of large relatively to small firms, might well result in less competition.

With this interpretation, the variance reflects the balance of power between firms in an industry and an increase in the variance would reflect a reduction in the scope for competition between them. Of course, a concentration ratio would provide the same answers, if the number of firms remained constant, for a growth in the size of the giants relatively to the small firms would result in an increase in the concentration ratio. But a concentration ratio may not reflect the balance of power within an industry if the number of firms changes. For example, if amalgamations occur among the small firms in an industry, the number of firms is reduced, the average size increases, the variance falls, but the concentration ratio, based on the largest three firms, may remain constant. Since the power of the small firms has increased as the result of amalgamations, they may exert a greater competitive threat to the giant firms. This increased scope for competition is reflected in the reduced variance but is not reflected in the concentration ratio. Thus the variance is a useful measure of some aspects of business concentration quite apart from its relationship to the concentration ratio. It can reflect the growth of large firms relatively to small firms, a major problem in the dynamics of business concentration.

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[1] This qualification is important, for the variance may increase without the growth of large firms relatively to small firms, but as a result of small firms overtaking the large. This case of increasing variance requires a different interpretation. *Cf.* pp. 240-3.

In some cases, knowledge of the change in the *number* of firms is also important. A fall in the number of firms to a critically low level say 10, would itself imply *increased* scope for oligopolistic situations. But a fall from 100 to 50 would not have the same implications. Thus the example of amalgamations just considered illustrates the complexity of the economics of concentration; in addition to a concentration ratio, it is necessary to have a measure of inequality and, in some cases, to know the number of firms. The upshot is that more than one measure is necessary before conclusions can be reached on the degree of competition within an industry.

Changes in Concentration

The dynamic analysis of business concentration is concerned with comparing the growth of large firms and small firms, with estimating the importance of ' external ' growth due to acquisitions or amalgamations, and with the size mobility of firms. Like the trees in a Marshallian forest, the firms being studied are continually changing in size: in addition, the forest is changing as the result of the appearance of new trees and the death of old trees.

The first problem to be considered in such a changing business population is the relative rate of growth of large and small firms. A lognormal distribution is generated if the chance of a given proportionate growth is the same for all firms, irrespective of absolute size. This is Gibrat's *law of proportionate effect*[1] and has been interpreted by Kalecki[2] as resulting in a continuous increase in inequality, in the absence of constraints.

A possible constraint is the effect of ' births ' and ' deaths '[3] which in the present business population means the effect of new quotations and of the loss of quotations among quoted brewing companies. Consequently, Table IV has been prepared to show the numbers, means and variances of surviving brewing companies during four periods between 1896 and 1950, together with similar measures for ' births ', ' deaths ' and the total number of firms.

[1] R. Gibrat, *Les Inegalités Economiques* (Paris: 1931).

[2] M. Kalecki, On the Gibrat distribution, *Econometrica*, 13 (1945).

[3] *Cf.* Hart and Prais, *op. cit.*, p. 166.

All values have been calculated from the transition matrices in the Appendix; the units are therefore in logarithms to the base 2, and the means are shown as deviations from £707,000.

It can be seen that the value of σ^2 for surviving companies increased in each period, in conformity with the above interpretation of the law of proportionate effect. However this increase was not regularly counteracted by the effect of births and deaths, as was the case in the whole of the quoted manufacturing sector.[1] Births increased the variance in 1896–1907 and 1907–24, but reduced it in 1924–39 and 1939–50. Deaths reduced it in 1896–1907, 1907–24, and 1939–50, but increased it in 1924–39. Nor was there any regularity in the net effect of both births and deaths on the relative dispersion of surviving companies. The data studied here do not support the view that the tendency for increasing inequality among a constant set of companies is reduced by birth and death effects, for this constraint occurred in only one of the periods, 1939–50.

On the other hand, the absence of regularity does not support the opposite view[2] that an increase in the number of firms in an industry always leads to an increase in inequality as measured by a Lorenz curve or the variance of the logarithms of firms' sizes. The change in the total variance, as the number of firms increases, depends on the mean and variance of the births, which are related to the mean and variance of the total number of companies as shown by the following equation:

$$\sigma^2 = w_1\sigma_1^2+w_2\sigma_2^2+w_1w_2(\bar{x}_1-\bar{x}_2)^2 \qquad (1)$$

In this equation σ_1^2 and $\bar{x}_1$ refer to the variance and mean of the logarithms of sizes of surviving firms, while w_1 refers to their proportion in the total number of surviving firms plus births. Similarly, σ_2^2, $\bar{x}_2$, and w_2 apply to births.

The conclusion on the rate of growth of large firms relatively to small firms is quite clear for surviving companies: in each of the periods, the variance increased.[3] The same conclusion applies to all quoted brewing companies until 1939, but in the period 1939–50 the means and variances of births and deaths were such that the tendency for inequality to increase among surviving companies was counteracted. Even in this exceptional period the variance decreased by very little, from 4.62 to 4.47, so that the main conclusion, that inequality of firm size increased over the period, remains substantially true.

It is interesting to note that the observed concentration ratios and concentration curves, in section 2, mask the rapid rate of growth of some large com-

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[1] *Ibid.*, p. 166.

[2] *cf.* J. M. Blair, Statistical Measures of Concentration in Business. BULLETIN, Vol. 18, No. 4 (Nov. 1956), p. 356 and p. 358.

[3] This increase in the variance is explained more strictly later. See pp. 240-3.

TABLE VI

Numbers, Means and Variances of Quoted Business Units in the Brewing Trade, 1896–1950

Units : $\log_2$

	1896–1907			*1907–24*			*1924–39*			*1939–50**		
	N	$\bar{x}$	σ^2	N	$\bar{x}$	σ^2	N	$\bar{x}$	σ^2	N	$\bar{x}$	σ^2
1. Total at 1st year	81	−0.86	2.59	132	−1.27	2.75	141	−1.41	3.70	166	−0.31	4.62
2. ‘ Deaths ’ in period	10	−1.60	2.84	32	−2.19	2.51	40	−1.93	2.75	28	−2.00	1.71
3. Survivors at 1st year ...	71	−0.76	2.46	100	−0.96	2.46	101	−1.21	3.93	138	0.04	4.51
4. Survivors at 2nd year ...	71	−0.69	2.61	100	−0.99	3.49	101	0.07	4.88	138	0.27	4.78
5. ‘ Births ’ in period	61	−1.98	2.02	41	−2.44	2.73	21	0.57	2.34	22	−0.77	1.63
6. Total at 2nd year	132	−1.27	2.75	141	−1.41	3.70	122	0.16	4.48	160	0.13	4.47

* Official and Supplementary Lists.

panies, simply because their values at any one point of time depend so much on the size of the largest firm, which may not be growing as rapidly as the other large firms. Thus for some purposes, including measuring the effect of changes in the business population and the rate of growth of large firms relatively to small firms, the statistical approach has the advantage.

Amalgamations and Inequality

The enumerative approach in section 2 revealed that amalgamations in this industry have had little effect on concentration ratios and, therefore, have not increased concentration. It is now possible to calculate the effect of all observed amalgamations on the variance of the logarithms of firms'sizes.[1] This measure reflects the balance of power in the industry: for example, if two large companies amalgamate the variance *increases*, but if two small companies amalgamate the variance *decreases*, because the first case results in a new business unit farther away from the mean size of all companies and the second leads to a new company nearer the mean size.

The statistics of quoted companies involved in amalgamation are given in Table VII: they have been calculated from ungrouped data[2] and the means have been transformed into deviations from £707,000 for purposes of comparsoin with Table VI.

TABLE VII

Statistics of Quoted Brewing Firms Involved in Amalgamations
Units : $\log_2$

	1896–1907		*1907–1924*		*1924–1939*		*1939–1950*	
	Before	*After*	*Before*	*After*	*Before*	*After*	*Before*	*After*
Number ...	7	3	12	6	26	13	14	7
Mean ...	−0.30	0.95	−1.24	0.40	−1.11	0.13	−1.83	1.06
Variance ...	3.45	2.66	2.76	0.53	3.42	2.47	4.71	2.19

The number of firms involved in amalgamations was about 9 per cent of all firms in the periods 1896–1907, 1907–24, and 1939–50, and 18 per cent in the period 1924–39. Though the rate of amalgamation in this industry was slightly above the average for all industries,[3] the number of amalgamations was still small compared with the total number of firms, with the exception of the period 1924–39. Thus the weights of amalgamated companies in equation (1), which is used to calculate the effect of amalgamations, were small and did not exert a large influence on the variance.

A comparison of the means in Tables VI and VII shows that firms involved in amalgamation were larger than the average size of all brewing firms in the periods 1896–1907 and 1924–39 and smaller than average in the other two periods. Table VII also shows that the obvious effect of amalgamations was to increase the mean size of companies. But the decrease in the variance in each period after amalgamation, also shown in Table VII, is not obvious and did not occur, for example, in the case of all industries in the period 1896–1907.[1]

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[1] *cf.* Hart and Prais, *op. cit.*, p. 168.

[2] It is worth noting that there were not significant differences between these results and those obtained from amalgamated companies classified into the class intervals of Tables A.1 to A.4.

[3] *cf.* Hart and Prais, *op. cit.*, p. 169.

TABLE VIII

Effects of Amalgamations on the Variance of Firms' Sizes.

	1896–1907	*1907–1924*	*1924–1939*	*1939–1950*
Total at beginning	2.59	2.75	3.71	4.62
Total after amalgamation ...	2.62	2.13	3.89	4.54

The effect of amalgamations on the relative inequality of the size distributions over time in shown is Table VIII. Amalgamations resulted in a slight reduction in the variance in the two periods 1907–1924 and 1939–1950, and led to a slight increase in the other two periods. Since amalgamations between large companies tend to increase relative inequality, it follows that this effect must have been outweighed in the periods 1907–1924 and 1939–1950 by amalgamations between small companies, or by amalgamations between large companies and small companies, which tended to reduce the variance. The fact that the means of companies involved in amalgamations in 1907–1924 and 1939–1950 were below the average of all brewing firms suggests that the reduction in the variance was due to amalgamations between small firms. Similarly, the increase in the variance in the periods 1896–1907 and 1924–39 may be attributed to amalgamations between large firms because the mean size of amalgamating companies was larger than average.

The conclusion suggested by this analysis is first that the amalgamations in the brewing industry have not generally occurred between large firms. This explains why concentration among quoted brewing firms, as measured by the concentration ratios in section 2,[2] has tended to fall in spite of amalgamations. Secondly, the slight effects on the variance, whether positive or negative, suggests that amalgamations were not an important cause of variations in the rate of growth of large firms relatively to small firms, and that most attention should be paid to internal growth. This confirms the conclusion of the enumerative approach in section 2,[3] but the statistical approach is undoubtedly a more elegant, and less arduous, method of assessing the effects of amalgamations on concentration.

Variations in Growth

It has been shown[4] that in a simple lognormal model, in which firms grow by a random proportion of their momentary size, the variance of the transformed distribution changes over time in accordance with the following equation:

$$V(x_{t+1}) = \beta^2 V(x_t) + \sigma_\epsilon^2 \qquad (2)$$

where the variances at two dates are denoted by V and time subscripts, the regression coefficient is denoted by β and the residual variance by σ_ϵ^2. That is, when firms grow by random proportions of their momentary sizes the relative dispersion of the distribution will tend to increase over time by σ_ϵ^2 unless the regression coefficient is below unity.

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[1] *Ibid.*, p. 169. [2] See pp. 229-30. [3] See pp. 231-3. [4] Hart and Prais, *op. cit* p. 172.

A possible economic interpretation of this concentration process relates to the theory of the optimum firm. If there is an optimum size of firm in the brewing industry, one would expect to find evidence of regression—a tendency for large firms and for small firms to become nearer the mean size. Over the past half-century the optimum size of firm may have increased so that large firms need not decline in size for regression to take place; it is sufficient that large firms grow less rapidly than small firms. This would be indicated by a regression coefficient of less than unity.

The observed values of the residual variance, the regression coefficient and its standard error, and the correlation coefficient have been calculated from the transition matrices in the Appendix and are given in rows 1–4 of Table IX. The residual variance and regression coefficient are also given in a form standardized for a ten year interval.

TABLE IX

Residual Variance, Regression and Correlation Coefficient

(a) *Brewing Calculated Values*	*1896–1907*	*1907–1924*	*1924–1939*	*1939–1950*
1. s^2 Residual Variance ...	0.75	0.68	1.41	1.12
2. b Regression Coefficient ...	0.87	1.07	0.94	0.90
3. S(b) Standard Error of b ...	±0.07	±0.05	±0.06	±0.04
4. r Correlation Coefficient ...	0.84	0.90	0.84	0.88
Standardised Values				
5. $\bar{s}^2$	0.69	0.39	0.96	1.03
6. $\bar{b}$	0.88	1.04	0.96	0.91
(b) *All Manufacturing Standardised Values*				
7. $\bar{s}^2_m$	1.29	1.04	2.01	1.24
8. $\bar{b}_m$	0.91	1.05	0.95	0.77

The first point to note is that the regression coefficients in each period are in the neighbourhood of unity. Though the standard errors in row 3 make it possible that the regression coefficients are below unity, this is not sufficient evidence to assert with any confidence that large and small firms have regressed towards the mean. In particular, it is worth noting that the final period 1939–1950 did not witness so strong a regression as occurred within all manufacturing companies quoted on the Stock Exchange: this can be seen by comparing the regression coefficient of 0.91 for brewing with the 0.77 for all manufacturing in Table XI. The absence of regression would mean that the variance of surviving firms increased in each period in accordance with equation (2) as shown in Table VI. But since this increase in the variance was not the result of the growth of large firms relatively to small firms, the explanation of diminished scope for competition, as outlined in section 3[1] cannot be adopted. The growth of the variance, σ^2, was the result of the cumulative effect of the residual variance, σ_ϵ^2, and may be explained along the following lines.

Even though large firms and small firms have the same average proportionate rate of growth, the relative dispersion of the distribution can increase as the result of variations in growth about that average. Such variations are measured by the residual variance, denoted by σ_ϵ^2 in equation (2). Spectacular successes and failures would be reflected in a high value of σ_ϵ^2. That is, it is a measure of the size mobility of firms; it measures the extent to which brewing companies overtake each other in growth and may therefore be interpreted as reflecting the intensity of competition.

With this (very tentative) interpretation, the increase in the residual variance in each period since 1907 would suggest that the intensity of competition has increased in this industry between 1907 and 1950 against a background of a fall in total beer consumption of 24 per cent over the same period;[2] in a declining, or stagnant, industry a firm can grow only at the expense of other firms, and the result is a high degree of size mobility. We should also expect to see an excess of deaths over births in such an industry. Unfortunately, the Stock Exchange data relate only to the birth and death of quotations, but it is interesting to note that deaths exceeded births in this quoted population in the periods since 1924,[3] which is consistent with our interpretation.

A further indication of the degree of competition in the brewing industry is provided by a comparison with industry as a whole. The variance of firms' sizes within a single industry is likely to be less than that for manufacturing as a whole;[4] the latter sector contains industries with a high average size of firms, such as the iron and steel trade, and industries with low average firm size, such as the cotton industry. Similarly, in the dynamic case, the variance of the proportionate growth of firms is likely to be less for a single industry than for manufacturing as a whole; a firm can grow by increasing the number of its products and thereby it can enter new industries. A comparison of rows 5 and 7 in Table IX shows that up to 1939 the residual variance of quoted brewing firms was about one half of that for all quoted firms in manufacturing. But in the final period, 1939–50, the brewing residual variance of 1.06 was only slightly below the 1.24 for all quoted firms in manufacturing. This suggests that the size mobility has not only increased over the whole period but that its present level is higher relatively to the total degree of size mobility.

However, this attempt to relate size mobility to the intensity of competition has obvious shortcomings: before any final conclusion could be reached about the degree of competition in an industry, it would be necessary to seek evidence of schemes for market sharing, price agreements, State regulations and other restrictions of competition. The most that can be said is that since all these restrictions are likely to affect the size mobility of firms, the residual variance should at least be considered as a possible summary measure of the effects of such restrictions on competition. Severe restrictions on competition make it more difficult for firms to overtake each other and therefore the residual variance would be low: conversely, a high residual variance would suggest that restrictions on competition were either not severe in intent or were ineffective. The statistical measurement of size mobility is undoubtedly superior to the enumerative method of trying to obtain an impression of size mobility by inspecting ranks at different points of time. But its elegance must not blind us to its limitations: it is a promising approach, but, like all measures of concentration, it is merely a preliminary to the detailed study necessary to determine the extent of monopoly or competition in any industry.

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[1] See pp. 236-9.

[2] *Cf.* R. Prest, *Consumers' Expenditure in the United Kingdom, 1900–19*, (Cambridge, 1954) Table 48, p. 76, and R. Stone, *The Measurement of Consumers' Expenditure and Behaviour in the United Kingdom 1920–38*, Vol. 1 (Cambridge: Table 68, p. 188).

[3] *Cf.* Table IX. This is confirmed by the declining number of brewers shown in L. McMullen United Kingdom Brewing Industry Statistics ', *J.R.S.S.* (1950), Vol. CXIII, p. 232.

[4] This is in fact the case, as can be seen by comparing the estimates of in Table IX with those in Table 8 in Hart and Prais, *op. cit.*, p. 166.

4. Conclusion

The business population studied in this paper consisted of brewing companies quoted on the London Stock Exchange between 1896 and 1950. The fact that each member of this population could be identified, and its size measured by market valuation, enabled us to calculate various measures of business concentration and to make a comparison between them. Though this was the main purpose of the paper, it is also possible to draw some subsidiary conclusions about trends in concentration in the British brewing industry as a whole, now that the quoted portion of the industry has been subjected to the enumerative and statistical methods of analysis.

Static Analysis

It is convenient to divide the comparisons of the approaches into those relating to the static analysis and those relating to the dynamic analysis of business concentration.

If the size distributions of brewing companies were exactly lognormal the enumerative approach and the statistical approach would provide the same measures of concentration at one point of time. But the estimates of the size of the three largest firms, based on a lognormal hypothesis, were not accurate enough to measure concentration at one point of time. To study the extreme upper tail of the size distribution, it is advisable to adopt the enumerative approach and use concentration ratios, or, better still, concentration curves. To measure the balance of power within the whole industry, the concentration ratio may be supplemented by the variance of the logarithms of firms' sizes.

However, in comparisons of concentration, the enumerative approach is not sufficient; concentration curves frequently cross when comparisons are made between different industries, or when concentration within one industry is being measured at different points of time. In such cases a summary measure of concentration is required and an appropriate measure is the variance of the transformed distribution.

Dynamic Analysis

The rate of growth of large and small firms, the size mobility of firms, and the effect of amalgamations, cannot generally be revealed by inspection or by the calculation of concentration ratios. In section 3, the average growth of large firms relatively to small firms was measured by a regression coefficient. The fact that this was in the neighbourhood of unity suggests that large firms *on the average* have not grown relatively to small firms. Neither have they grown less rapidly than small firms, so there is no strong evidence for any tendency towards an optimum size of firm.

In the absence of regression, the variance of surviving firms has increased in each period, and this increase has not been constrained by births and deaths. The continuous increase in the variance cannot be explained by amalgamations, nor by the growth of the large firms on the average relatively to small firms. Relative dispersion of the sizes of brewing companies has increased, in spite of large and small firms having the same average rate of growth, as the result of the dispersion of growth about that average rate. Some large firms and small firms had growth exceeding the average: the former overtook the previous leaders, and the latter ousted the large firms with less than average growth from the ' giant ' category. The appearance of new and larger leaders in the upper tail of the size distribution, together with the opposite process which occurred in the lower tail of the distribution, resulted in an increase in the variance of the distribution. Thus the increase in inequality must be attributed to size mobility rather than to the growth of a constant set of large firms relatively to small firms.

Concentration in the Brewing Industry

Though the Census disclosure rules make it impossible for the outside commentator to apply the various measurements to the brewing industry as a whole, it is possible to apply them to quoted brewing firms. The analysis in sections 2 and 3 suggests that the proportion of the brewing business controlled by the few firms at the top is low and has been decreasing.

On the other hand, the inequality of size of firms among surviving breweries has increased regularly since 1896. But since this increase was due to size mobility, and not to the growth of large firms relatively to small firms, it could be attributed to the intensity of competition between brewing firms. Furthermore, size mobility, as measured by the residual variance, has increased over the whole period. Thus the enumerative and statistical approaches suggest that the British brewing industry is competitive: but this conclusion must remain tentative until a more descriptive analysis of this particular industry is undertaken which could provide further tests of the techniques employed in this essay.

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APPENDIX

THE SIZE DISTRIBUTION OF QUOTED BUSINESS UNITS IN THE BREWING INDUSTRY IN THE UNITED KINGDOM, 1896–1950

Business units engaged mainly in brewing and quoted in the London Stock Exchange category, *Breweries and Distillieries*, were identified after eliminating the relatively few units engaged mainly in distilling. A business unit is defined as a parent company plus all subsidiaries in which it has more than 50 per cent voting power. Subsidiaries not engaged in brewing were excluded.

For 1935 and 1951 the net tangible assets of these business units were calculated from the account summaries published by Moody Services, London, E.C. 4. Net tangible assets equal total assets (net of depreciation) less current and deferred liabilities and provisions.

In calculating the market valuation of quoted brewing firms, market prices at the end of March in each chosen year were taken from the Official List 1896–1939 and from the combined Official and Supplementary List in 1939 and 1950. Ordinary, preference and loan capital of a business unit were included in the valuation. With the identification and measurement of each business unit, it was possible to construct transition matrices as shown in Tables A1 to A.4.

TABLE A.1

Transition Matrix by Market Valuation of Quoted Business Units engaged in Brewing, 1896–1907

	A	B	C	D	E	F	G	H	K	L	M	N	O	P	Q	*Total*	*Births*	*Total 1907*
A																		
B																		
C																		
D																	1	1
E					1		1									2	4	6
F						4	4									8	15	23
G							6	5								11	21	32
H						2	6	7	4	1						20	14	34
K							2	6	6							14	2	16
L									3	5	2					10	2	12
M										1	2	1				4	2	6
N											1					1		1
O													1			1		1
P																		
Q																		
Totals ...					1	6	19	18	13	7	5	1	1			71	61	132
Deaths ...						4	3	1		1	1					10		
Total 1896					1	10	22	19	13	8	6	1	1			81		

Table A.2.

Transition Matrix by Market Valuation of Quoted Business Units in Brewing 1907–1924

	A	B	C	D	E	F	G	H	K	L	M	N	O	P	Q	*Total*	*Births*	*Total 1924*
A																		
B																		
C																		
D																		
E					1	4										5	13	18
F						10	13									23	13	36
G							7	5								12	5	17
H							7	13	3							23	2	25
K								4	10	1						15	7	22
L							1	2	1	6	4					14		14
M										3	1					4		4
N										1	1					2	1	3
O												1				1		1
P													1			1		1
Q																		
Total					1	14	28	24	14	11	6	1	1			100	41	141
Deaths			1		5	9	4	10	2	1						32		
Total 1907			1		6	23	32	34	16	12	6	1	1			132		

Table A.3

Transition Matrix by Market Valuation of Quoted Business Units engaged in Brewing, 1924–1939

	A	B	C	D	E	F	G	H	K	L	M	N	O	P	Q	*Total*	*Births*	*Total 1939*
A																		
B																		
C																		
D																		
E					1											1		1
F					6	5	1									12	1	13
G						14	1	2								17		17
H					1	2	6	2	2							13	3	16
K						2	3	8	3							16	6	22
L					1	3	2	2	6	2						16	8	24
M								2	5	6						13	1	14
N							1			1	3					5		5
O									1	1		2				4	2	6
P											1	1	1			3		3
Q														1		1		1
Total					9	26	14	16	17	10	4	3	1	1		101	21	122
Deaths					9	10	3	9	5	4						40		
Total 1924					18	36	17	25	22	14	4	3	1	1		141		

TABLE A.4

Transition Matrix by Market Valuation of Quoted Business Units engaged in Berwing 1939–1950

	A	B	C	D	E	F	G	H	K	L	M	N	O	P	Q	*Total*	*Births*	*Total 1950*
A																		
B																		
C						1										1		1
D																		
E					2											2	1	3
F			1		1	4	5	1	1							13	2	15
G						1	12				1					14	1	15
H						3	3	15	4							25	8	33
K							1	1	9	2						13	7	20
L						1		2	10	17	1					31	3	34
M						1			2	6	11	2				22		22
N										1	3	1				5		5
O											1	1	6			8		8
P														3		3		3
Q															1	1		1
Total			1		3	11	21	19	26	26	17	4	6	3	1	138		160
Deaths					4	6	9	5	3	1						28		
Total 1939			1		7	17	30	24	29	27	17	4	6	3	1	166		

The upper limit of class K is £1 million and the upper limit of every class is twice the lower limit. This is a simple way of compiling bivariate frequency distributions of the logarithms of the sizes of firms, for in logarithms to the base 2 each class interval equals unity.

TABLE A.5

Deviation from Log normality of Size Distributions of Quoted Business Units in the Brewing Trade

	1896	*1907*	*1924*	*1939*	*1950*
g_1 ...	0.65 ±0.27	0.41 ±0.21	0.66 ±0.19	0.14 ±0.19	0.35 ±0.19
g_2 ...	0.09 ±0.53	0.37 ±0.42	0.13 ±0.39	−0.03 ±0.38	0.01 ±0.37

The size distributions of firms at points of time are given by the final rows and final columns of each matrix. These have been subjected to two tests of lognormality. The first was a graphic test: when plotted on log-probability paper, these distributions were approximately linear and thus supported the hypothesis that the size distributions are approximately lognormal. The second test consisted of estimating the skewness and kurtosis of the transformed distributions by calculating Fisher's g_1 and g_2 statistics[1] shown in Table A5. The deviations in each year from zero kurtosis, within two standard errors of the g_2 statistic, were not significant at the 5 per cent level. But the same was not always true for the g_1 statistic, which measures skewness. With plus or minus two standard errors as the limit, it will be noted that the deviations from zero skewness were statistically significant in 1896 and 1924. The small, positive values of g_1 imply that the distributions were slightly more skew than the simple two-parameter lognormal distribution, and this may be explained in part by the fact that the quoted sector of the brewing trade contained relatively few of the small brewing firms. Even so, the amount of skewness was so small[1] that the lognormal curve may still be regarded as a good approximation to the observed size distributions of quoted brewing firms.

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[1] *cf.* R. A. Fisher, *Statistical Methods for Research Workers* Edinburgh: 1941), pp. 68–73.

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[1] Before logarithmic transformation g_2 would have at least the following values: 8.21, 11.2, 15.6, 33.5, 33.5.

MEASURING BUSINESS CONCENTRATION

A REJOINDER TO MR. BLAIR

By S. J. PRAIS

Mr. Blair's recently published article in this BULLETIN (November, 1956, contains so much good sense that it may seem ungracious to cavil at some of his points. He has however taken issue explicitly with a comment in a paper read by Mr. Hart and myself to the Royal Statistical Society last year[1] and, since I still cannot bring myself to accept his view on the merits of absolute *versus* percentage concentration ratios, perhaps I might venture a few remarks by way of rejoinder.

1. May I begin with an easy question. Mr. Blair asks (p. 368) can it be contended ' that the proper measure of concentration is one which shows concentration to be non-existent in an industry with only two firms, which would be the showing of the Lorenz curve where each firm produces half the output? ' If a simple answer has to be given it is Yes, but of course one cannot agree with what is implied in the question, and for two reasons. First, if there are only a few firms in an industry there seems little point in summarising the size distribution by means of a Lorenz curve or otherwise. Secondly, I do not think it can be taken for granted that there exists a single ' proper measure of concentration '. I would contend that, in the present state of economic science, two questions have to be asked of an industry: (A) How many firms are there? (B) What is the dispersion of their sizes?

In the present example the answer to A is sufficient for the industry to be referred to the Duopoly Department for action; only if there is a larger number of firms in the industry is there any need to proceed to question B. In that case it appears that Blair would be willing to take a further step and combine the answers to A and B into a single measure (say, C) which is some form of absolute concentration ratio. Now he may be right or wrong in the choice of his particular way of combining these answers; in any case it takes the argument to a different level of analysis, and it is difficult to see how there can in principle be any conflict with those concerned solely with the earlier stage of analysis.

Blair seems to suggest (see pp. 356–6) that a choice has to be made between measures of the type B and measures of the type C; whereas it seems to me that the choice is between a *single* measure of type C on the one hand, and the two measures A and B on the other. Precisely how A and B should be combined in order to provide policy recommendations seems to me a matter on which no one would wish to be dogmatic; I certainly see no reason to beg this question by always using an absolute concentration ratio, unless of course there are limitations in the availability of data.

2. Mr. Blair questions (pp. 367–8) the appropriateness of a measure

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[1] *J.R.S.S.* A, 119, 150. The passage quoted by Blair is from p. 153 (not, as Blair says, p. 151); there is also an error in the quotation—he quotes *ratio* as *ration*.

such as ‘ the Lorenz curve, which as between two industries of identical absolute concentration, shows higher concentration in one than in the other merely because of a greater number of firms ’. Suppose industry X has a hundred firms of which ten control half the output, and industry Y has twice as many firms in total but half the output is still controlled by ten firms. It then seems to me quite consistent with normal usage to say that Y is more concentrated. Perhaps Blair would prefer, as do some other writers, to say that ‘ inequality ’ is greater in Y. If so, then it is only a matter of words between us.

However, Blair goes on to suggest that there is a sense in which concentration is *lower* in Y; the Lorenz curve produces a measure which he says (p. 368) is influenced ‘ in the wrong direction from the point of view of economic theory ’ (surely ‘ wrong ’ is wrongly used here). I would prefer to say that, despite the *higher* concentration (or inequality of sizes) in Y, the degree of competition may be greater than in X because of the greater number of firms. If I do not misinterpret him, Blair agrees that this proposition is true (indeed, a truism, see p. 367; but I am surprised he thinks it an ‘ irrelevant truism ”.)

In practice, therefore, I think there is perhaps not much between us, though we may use *very* different words in describing the same situation. In matters of theoretical analysis, however, I am convinced that it is a mistake to work entirely with absolute concentration ratios, and to throw overboard the statistician’s knowledge of normal curves, sufficient statistics, the analysis of variance, etc., which can be employed with advantage in analysing long-term trends in concentration.

3. I do not doubt that there are many advantages in depicting the size distribution of firms in the form of a ‘ cumulative concentration curve ’, as advocated by Blair. It is odd to find, however, that Blair is still (see p. 370) commending to statisticians the task of finding a mathematical description of this curve without mentioning—or, at least, giving us his views on—any of the work on the lognormal distribution, due in this connexion mainly to Gibrat and Kalecki, and further work by Aitchison and Brown, Tuck, Rutherford and Simon. The lognormal distribution is certainly not entirely adequate, but it is the best simple curve put forward so far. It depends on two parameters for its description (the number of firms and the variance of the logarithms of the sizes; the third parameter, the mean, is absorbed as a scale factor); hence, as already suggested above, it will not be possible to arrange industries along a one-dimensional scale indicating, shall we say, the strength of the tendency towards monopolistic practices, unless we have an ‘ indifference function ’ between these two variables.[1]

[1] Though it may seem to come a little close to point-scoring, I add a further point on a formal matter. Blair suggests in his opening paragraph on Concepts of Concentration (p. 351) that, in statistical terms, concentration is concerned with the number of units and the *skewness* of the distribution. I think, on the contrary, that it is the number of units and the *dispersion* of the distribution (the second, rather than the third, moment) that have occupied the attention of most analysts. I suspect that this is not a slip on Blair’s part, but a fundamental misunderstanding which leads to the difficulties that are the subject of the present note.

In summary: I cannot agree that there is any substance in the view that there is a ' *legerdemain* of the Lorenz curve ' which ' beclouds and obfuscates ' the issue, and ' may present the economist with false and misleading information ' (p. 356). It may have done all these things, but the fault lies not in the tool.

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FURTHER REJOINDER

By J. M. Blair

By clarifying a number of points Dr. Prais' comments have served the useful purpose of narrowing somewhat the difference between us. But an important difference remains. It stems from his failure to recognize or accept the *economic* reasoning underlying my position. Perhaps I can make this reasoning somewhat clearer by starting with the illustration, which I previously cited, of the industry of only two firms, each accounting for half of the output, where of course the Lorenz curve shows no concentration. But if it were assumed that the two firms accounted for 90 per cent, rather than 100 per cent of the output, with the remaining 10 per cent being distributed among 8 small firms, the Lorenz curve would show a moderately high level of concentration (90 per cent of the output produced by 20 per cent of the firms). Moreover, if it were assumed that that 10 per cent is shared by 98 small firms, the Lorenz curve would show a still higher level (90 per cent of the output accounted for by only 2 per cent of the firms). And so on *ad infinitum*. Two conclusions suggest themselves. First, any measure of economic concentration which *ceteris paribus*, rises with an increase in the number of firms is indeed moving in the wrong direction. Second, there exists in most industries a considerable number of very small enterprises which exert little, if any, influence on the industry's behaviour with respect to price, production, etc. Any measure of concentration which fluctuates with changes in the *number* of these tiny enterprises is not meaningful from an economic point of view. This latter consideration is of particular importance in differentiating oligopolistic from non-oligopolistic industries. There, the question is whether the market is substantially controlled by the few. To use a measure such as the Lorenz curve which, as between two industries of identical control by the few, show different levels of concentration merely because of a difference in number of enterprises impedes rather than facilitates economic analysis.

As to his other observations, some I would accept, others reject. None appears to be sufficiently important, however, to warrant further discussion at this point.

Washington, D.C.

Dr. NEVIN AND THE WELSH ECONOMY

By J. Parry Lewis

In recent years a renewed interest has been shown in the state of the Welsh economy, and much effort has been devoted to producing statistics.[1] The latest contributions come from Dr. Nevin who circulated his preliminary edition of *The Social Accounts of the Welsh Economy*, 1950 in October 1955. Some of this work was summarised in this Bulletin in February 1956[2] and in October 1956 he published a more final and detailed report on his calculations.[3] Unfortunately, however, a careful examination of Dr. Nevin's paper reveals that some of the statistics he prints are not quite what he calls them; and some of the conclusions he reaches are quite invalid. Since it is only in this paper, containing the preliminary results of his work, that he comments on the implications of his figures, it is this paper which must first be examined. Afterwards we shall consider whether the revised calculations call for any modification of our criticisms.

It will be convenient to divide the paper into the following sections:

1. An examination of Dr. Nevin's conception of a residual error, and the consequences of this on his calculation of 'net exports' in his first publication.
2. An examination of his note on the Structure of the Welsh Economy.
3. An examination of his analysis of the female labour situation.
4. An analysis of his revised estimates.

The Residual Error and 'Net Exports'

Dr. Nevin is the first person to attempt to calculate the Welsh G.N.P. from both the income and the expenditure side, and a summary of his results appears in his Table I in the Bulletin. The detail of his calculation is given in *The Social Accounts of the Welsh Economy*, 1950, of which this table is a summary. Three items in this table call for special comment, both because they are relevant to the subsequent discussion and because they shed some interesting light on Dr. Nevin's method of working.

On the expenditure side of the table is an item called 'net exports'.

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[1] The first examination of the Welsh Incomes was Phyllis Deane's *Regional Variations in United Kingdom Incomes from Employment*, *J.R.S.S.* (*A*) Vol. CXVI, Part II, 1953, pp. 123–139. About the same time, Miss Anne Martin, of Cardiff, drew up a scheme for the calculation of the Welsh G.N.P. from the income side. After completing a calculation of the Agricultural Labour Bill she put aside her calculations to await the publication of the Census of Production results, and devoted her activities to compiling *Welsh Economic Statistics: A Handbook of Sources*, which was completed by myself and published by this Department in 1954. This initiated a full-scale study of the Welsh economy which is still proceeding at this College and will shortly result in a book on the subject. Meanwhile, my own estimates of the Welsh G.N.P., based on Miss Martin's work appeared in a preliminary form in *The Western Mail* on August 1st, 1955.

[2] Bulletin, Vol. 18, No. 1, February 1956, pp. 61–71, in which a note draws attention to the availability of the detailed methods, previously published under the title given in the text.

[3] *The Social Accounts of the Welsh Economy, 1948 to 1952,* Edited by Edward Nevin, University of Wales Press, 1956.

Calling this an 'especially hazardous estimate', the author indicates that its range of error is greater than twenty per cent. On the income side is an estimate of 'net income from abroad' for which no indication of the possible error is given. The total of expenditures comes to £434 million, and the total of incomes to £433 million. There is a balancing item, called 'residual error' of £1 million.

All this looks very proper until one reads that the figure for net exports has been obtained as a residual. We then find that we have a table containing two residuals each, apparently, derived from the table itself. One of the residuals is called 'error' and the other is called 'net exports'. It is puzzling, and the BULLETIN paper provides no clue to the solution. For this we must turn to *The Social Accounts*, 1950, and here we find that both Stock Appreciation and Residual Error have been calculated by taking 4.1 per cent of the corresponding U.K. terms simply because the Welsh G.N.P. has been estimated at 4.1 per cent of the U.K. G.N.P. This, of course, is a questionable technique when applied to stock appreciation; but when applied to residual errors it becomes remarkable. The table must, if it is to mean anything, be reprinted with the two sides totalling £433, the expenditure side showing a term of £33 million, described quite clearly as 'Residual error plus net exports'. And until some remarks have been made about the possible size of the residual error it is pointless to attempt to ascribe any magnitude to the net exports. We shall make these remarks in a moment.

Before doing so, however, we must consider the derivation of the term 'net income from abroad'. This seems to have been derived from the net values of income from employment paid to persons resident outside Wales; incomes from self-employment generated inside Wales by persons living outside; and property incomes arising in Wales but being paid to outsiders. Each of these items is, of course, exceedingly difficult to calculate, and Dr. Nevin could not have been blamed if he had said so and left it at that. Instead he has made an estimate which, though he claims no great accuracy for it, he considers good enough to print. And because it plays an important part in this table we must consider it in some detail. Perhaps it should be pointed out that in the revised estimates this item is obtained in a different way; but the remarks we are about to make have a bearing on this.

It is possible to make an estimate of wages and salaries generated in Wales by considering the results of the Census of Production and other inquiries. In this way an estimate of £320.4m. has been made. I have no quarrel with this estimate, except to point out that, if only because of the peculiar definition of the Census of Production year of return, to say nothing of the more obvious difficulties associated with industries not covered by the Census, the error in this figure could quite easily be as large as £15 million either way.

It is also possible to make an estimate of the wages and salaries received by Welsh residents by considering Income Tax data. Dr. Nevin has taken the Inland Revenue figure of £262 m., representing wages and salaries in

1949–50 of people whose main place of assessment was Wales, and has adjusted it to allow for non-industrial civil servants and merchant seamen. This figure has then been multiplied by 1.065, because this is the ratio in which income assessed under Schedule E rose between 1949 and 1950 in the U.K. as a whole. Finally adjustments were made to allow for pay in cash and kind of Armed forces and for Employers' contributions. The result of this is a figure of £317.1 million.

The figure of £317.1m. is then taken from the figure of £320.4m., obtained by the other method, to obtain an estimate of income from employment paid to persons resident outside Wales. Now quite apart from anything else, no allowance is made, in obtaining this smaller figure, for incomes under £135, although it would take only a few thousand old age pensioners earning less than two pounds a week, or young workers, or students showing some unaccustomed vigour during the vacations, to add a million or two to this sum. Furthermore, no attempt has been made to deal with the problem of multiple assessment.[1]

When we consider both the adjustments that have been made and those that should have been made, and the fact that the Income Census was, in any case, a sample enquiry, it is clear that once again we have to admit that there are possibly large errors in the total, and a five per cent error is quite easily imagined. In this case, if we consider that both the figure of £320.4m. and the figure of £317.1m. may have a five per cent error, we see that the difference between them may be anything between £(336.4–302.0)m. on the one hand and £(304.4–332.2)m. on the other, i.e., between +£34.4m. and —£27.8m.

The second component, concerning incomes from self employment is open to similar objections. It is the difference between one estimate of £41.9m. and another of £39.5m., and must be taken as anything between —£1.6m., and +£6.4m. Finally we come to property incomes, which is printed as a net item of £18.0m., but which, for similar reasons, could be anything between about £10m. and £26m.

We now come to add up. Dr. Nevin gets a total of £(3.3+2.4+18.0)m. = 23.7m. But in fact it should be anything between —£19m. and +£67m. when the above argument is considered.

Having made these points we are in a position to estimate the possible size of the residual error. If we attribute to each item the range of error that Dr. Nevin himself considers to be possible, and allow for the possible error in the figure for 'net income from abroad' (taking care not to make the same allowance twice) we find that the total expenditure, net of exports, could be anything between about £358m. and £582m. and the total income anything between £345m. and £523m.; and if these are independent estimates (as they should be) then the figure for net exports could be anything between —£237m. and +£165m. It is this figure that Dr. Nevin prints as —£34m. I do not suggest for one moment that net exports were as low as —£237m. But there is nothing in his calculations to prevent this. The whole point of this already over long demonstration is that the residual is not a thing to be taken as a convenient percentage of the U.K. figure. It is a very real problem. And the figure of £33m. must be called 'residual error plus exports'. It may very well be that if the truth were known (which it is not) we would find that the residual error was substantially larger than the net exports, and possibly of a different sign. On occasion no amount of ingenuity can overcome a paucity of information and the only reliable statement to make about net exports is that we cannot calculate them.

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[1] People living in country A and drawing two incomes, one assessed in country A and the other in country B, will have their total income recorded in the table for country in which their larger income is assessed.

The Structure of the Welsh Economy

Table IV of the Bulletin paper prints the gross output per head by industrial sector, and compares the Welsh values with those for the U.K. These values are then used to estimate what the level of Welsh production would be if Wales had an average productivity in each sector equal to the U.K. average productivity in that sector. It is concluded that the Welsh production would increase by about 14 per cent.

Basically there is nothing wrong with this approach, but for two reasons it would be unwise to place much reliance on the result that has been obtained. First of all, it must be remembered that many of the Welsh figures for Gross Output per head have, in fact, been obtained by taking certain proportions (sometimes quite arbitrary proportions) of the corresponding U.K. figures, or in some other way assuming some sort of equivalence between the two economies. For instance, because about three per cent of the U.K. banking, insurance and labour force was located in Wales, it has been assumed that the Welsh wages and salaries were three per cent of the U.K. figure, and that the gross profits, rent and net interest were similarly related to the U.K. total. Much more serious is the fact that in the case of manufacturing industry, the Census of Production gives little or no Welsh information for about forty industries. In these cases, Ministry of Labour employment figures have been expressed as percentages of the U.K. employment figures, and these percentages used to estimate the Welsh wages and salaries from the U.K. wages and salaries. Gross trading profits have then been calculated by assuming that the relationship between wages and salaries on the one hand and gross trading profits on the other, which is revealed by the Inland Revenue as holding for certain broad groups in the U.K. as a whole, also hold in Wales. One could go on giving examples, but enough has been said to indicate that if there is little information about some Welsh industry the chain of accumulative approximations is quite likely to indicate a productivity in that industry that is quite different from the truth.

Quite apart from this point, a possible error arises out of the use of broad industrial sectors. Let us, for example, consider the larger establishments in manufacturing industry. In 1950 the Census of Production published

Welsh information for 77 different industries. In these the average Welsh output per worker was £613 and the average U.K. output was £581. Now we could argue from this that if the U.K. productivity in this *sector* (defined, for the purpose of this example, to consist of the 77 industries for which Welsh information is given) were the same as the Welsh productivity, then U.K. output would *rise*. This, in fact, is the argument that Dr. Nevin has used, except that he has repeated it for several broad sectors, and then added the results. It happens, however, that of these 77 industries, 57 had a lower productivity in Wales than in the U.K., 19 had a higher productivity, and in one case there was no difference. If we now perform the kind of calculation for these separate industries that Dr. Nevin has performed for the sectors, we find that if the U.K. productivity in each *industry* were the same as the Welsh productivity then the total U.K. output of the industries concerned would *fall* by about 12 per cent. Here we have a case where attention to detail has led to a reversal of the result obtained by broad considerations. If we are going to talk about the economic structure of a country it is just not good enough to talk of broad sectors; individual industries must be considered. To do that involves much more work, and also involves making estimates of the per capita output in many industries for which little information is published. Only if reasonably reliable estimates can be made is there point in proceeding with calculations of this kind: if they cannot, then there is little point in using an analysis by sector, which is far too crude.

Female Employment

Dr. Nevin asserts quite rightly that the percentage of females employed in Wales is lower than elsewhere in the U.K. And he is right to attribute part of the lowness of the per capita output to this factor, which is one of concealed unemployment. But he is wrong when he states that because of the ageing population ' the demographic trend will operate against an increase in the occupied female population '. Female employment is low in Wales chiefly because there has been lack of opportunity. A hundred years ago, Welsh agriculture employed its fair share of women. Although these worked largely in the farm house, there was no stronger hostility towards the idea of women working than there was elsewhere in the U.K. But the growth of an economy based on heavy industry attracted the men from the land. The trek to coal and iron was a journey which was very largely for men only. Women whose husbands were unable to support them and were unable to run the farms themselves, usually went into domestic service, often in England. Not until light industry came to Wales was there much opportunity for women to work, except as domestic servants, about whom statistics are notoriously unreliable. Even to-day it is opportunity that is lacking, not women.

Now it is quite right that the Welsh female population is ageing, and that the number of women in the ' active element ' is going to decrease. But this is also true in the U.K. as a whole. As the years go by and the number of women able and ready to work diminishes throughout the United Kingdom, those factories which want female labour are naturally going to look to those parts of the country where unemployed female labour is most plentiful. The ageing population will put Welsh female labour at a premium. In Wales, the demographic trend will operate in favour of increased opportunities for women, and a consequent diminution in the present concealed unemployment of women who are out of work because there has never been work for them.

The Revised Estimates

The criticisms of the last two sections, dealing with the structure of Welsh industry and female employment are in no way altered by the revised estimates, and we shall say nothing more about these matters.

The first point that strikes one when comparing the revised estimates with the earlier ones is the extent of the revision. For example, whereas the earlier estimates suggested that Welsh personal incomes in the range £135–249 amounted to £68m. in 1950, the revised estimates suggest that personal incomes in the rather wider range of £0–249 came to rather smaller total of £50.5m. Comparison of the notes attached to the two sets of estimates give no indication of the reason for this. Another example comes from the table on Consumers' Expenditure, where a total of £374m. has been changed to £425m. The estimate for gross fixed capital formation in 1950 has been changed from £71m. to £96m.

Some of these differences arise for reasons that are difficult to detect. For example, the estimated expenditure on food has been changed from £113m. to £134m. although the notes give no indication of any difference in method. Other differences arise because improved methods are used, such as in revising the expenditure on alchohol from £22m. to £32m.

There are many points on which the revised estimates may be criticised in detail. Some of the more substantial of these arise out of the tables on capital formation. But it is time to consider whether the alterations in the tables have any effect on the validity of the criticisms we have already made of the conclusions drawn from the earlier estimates.

In order to answer this we must consider the six summary tables which appear in the front of the later publication. These tables show the Gross National Product (Table I), Personal Income and Expenditure (Table II), Coporate Income (Table III), Local Authority Revenue and Expenditure (Table IV), Transactions with the Rest of the World (Table V), and a Combined Capital Account (Table VI). It will be convenient to deal with the tables in an order other than that in which they are printed, since an appreciation of the later tables is necessary in order to assess the worth of the Table I. First of all, however, it should be pointed out that in his introductory notes, Dr. Nevin draws attention to the dangers of a residual. He explains that if, for example, ' personal income is estimated as £105 million and personal expenditure as £100 million, the estimate of personal saving becomes £5 million. If the correct figure for personal expenditure is now discovered to be £95 million, estimated personal saving becomes £10 million i.e., an error of 5 per cent in the original estimate for personal expenditure has resulted in a change of 100 per cent in the estimate for personal saving. Because of this unreliability, then, we have taken the precaution of having items derived as residuals printed in bold-faced type '. In fact about a third of the items derived as residuals are so printed.

No comment will be made about Tables III and IV, except that the first contains a residual item called ' Undistributed Income ' which is not printed in bold-faced type, and that the second contains a residual item called ' Surplus ' which is.

Table II is interesting because it reveals a different approach to the problem of incomes generated in Wales *versus* incomes paid to Welsh residents. After using the Census of Production and other sources to calculate the income generated by Welsh production and paid out in the form of wages and salaries as £325 million, Dr. Nevin proceeds to calculate the income going to Welsh residents in the way described in the following note.

' The totals from Table VII (*i.e.* incomes generated) after adjustments for (a) incomes of persons resident in Wales but employed elsewhere, and (b) incomes of persons employed in Wales but resident elsewhere. Available data suggests that (a) outnumber (b) to a small degree, so employment incomes have been increased by 1 per cent to allow for this. No adjustment considered necessary for self-employed persons '.

It will be noticed that no reason has been given for the choice of one per cent, although it is interesting to find that this results in an addition of £3m., which is the sum arrived at under this heading by the old method, criticised above. Furthermore, no indication of the character of the ' available data ' is given. *The Report on Usual Residence and Workplace* of the 1951 Population Census, which possibly appeared too late for Dr. Nevin to use it, is not very helpful, because of the inadequacy of the data. There were 6,300 persons stated to be resident in Wales and working in England, and 4,900 persons stated to work in Wales but reside in England. Although these are by no means comprehensive figures they tend to suggest that the adjustment of 1 per cent (implying a *net* movement of about 10,000 persons) may be rather high.

More serious is an error in the figure of net remittances abroad. This has been calculated by taking the number of aliens resident in Wales as a percentage of the number in the U.K. and then computing this percentage of the Blue Book figure. But what of the Englishman working in Wales who remits to his family. Is not this a remittance abroad? What of the Welshmen living in England? And what, indeed, of the Welshmen overseas?

The table shows that total personal incomes in 1950 came to £493m. Even if we ignore the points just made, and consider Dr. Nevin's indications of the accuracy of his figures to be correct, then we must consider this to have a possible error of as much as £25m. The estimate for current expenditure is £478m., which again must be considered to have a possible error of as much as £24m. The difference between them of £14.6m., is called ‘ Saving before providing for depreciation and stock appreciation ’. It is also before providing for error. Saving could very easily be anything between +£64m. and —£34m. I do not suggest that, in fact, these extremes are likely: but there is nothing in the calculations to forbid it.

We now turn to Tables V and VI. Before considering them we must consider what the Introductory Notes have to say about them. First we are told

> ‘ These two accounts . . . are prepared, in the main, for technical reasons associated with the checking of the consistency of the estimates and contain items which have all appeared in other accounts.’

We are then told that if an economy has relations with outside countries, then the Combined Capital Account is going to show an item of Net Investment Abroad. Also, the Table showing Transactions with the Rest of the World is going to have an item called ‘ Net Investment Abroad ’,

> ‘ which should, in theory, check with the same item in the capital account; if so then all is well. An absence of equality, however, would signify that something was amiss; hence the value of the last two accounts as a checking device.’

At the risk of seeming incredibly dull I must confess that I interpret this to mean that Table V gives one estimate of net investment abroad, and that Table VI gives another, obtained by some different route, ‘ which should in theory check ’. And because of that I am puzzled when I find it stated quite plainly in Table VI that the item of net investment abroad has been *derived from* Table V. It is not very surprising that they check.

Table VI consists entirely of items drawn from other tables, several of them residuals. However, wrong these may be, the Table is bound to balance, because of the method of construction.

We must, however, consider Table V in greater detail. The Debits side shows ‘ Net imports of goods and services from other U.K. regions ’ analysed into five groups by industry of origin, and then totalled to give ‘ Total net imports from the rest of U.K.’; ‘ Net imports from overseas ’; net remittances abroad; and Central Government Revenue Surplus and net property income payments. On the Credits side we have ‘ net exports of goods and services to other U.K. regions and overseas ’ analysed into seven groups and totalled to give ‘ Total net exports ’; and ‘ Net income from abroad ’ divided into property income and employment income. The difference between the two totals is called ‘ Net Investment Abroad ’. It is an impressive table, but unfortunately there is not a single reliable figure in it. This is particularly important because, not only is this table related to Table I, the key table, in a mysterious way which we shall consider in a moment, but also because it has already attracted considerable public comment.

First we may dispose of some of the smaller items. ‘ Net remittances abroad ’ is wrong for the reason we have already pointed out. ‘ Central

Government Revenue Surplus ' is a residual from a table which includes such questionable items as expenditure on defence. ' Net property income payments ' includes Central Government Trading Income (which is admitted to be an arbitrary figure), rents, and National Debt interest, all of which involve large sources of possible error (although it must be admitted that any error in these will be reflected by a corresponding change in the residual Revenue Surplus). We also find that net employment income from abroad is the arbitrary figure we have previously criticised, and that the figures for net property income from abroad are very difficult to obtain by adding the items mentioned in the notes.

We are left with only the imports and exports. When one considers the problems of measuring the volume of Welsh imports and exports across the national border one sees that any direct approach to the problem is virtually impossible. Dr. Nevin's estimates are explained by the note:

> ' Distribution of exports and imports estimated indirectly using a form of input-output analysis. In the Blue Book inter-industry relationships appear for the years 1948 and 1950; values of U.K. input coefficients obtained from these. Welsh gross outputs estimated from Census of Production. U.K. coefficients applied to these to complete the body of the table. Final demand sectors completed using social accounting data. Exports and imports then obtained as residuals.'

I must confess that I do not know of any other way in which estimates of Welsh imports and exports could be made. On the other hand, I do feel that the accumulation of approximation and implicit assumption reduces these figures to nothing more than an ingenious exercise in imaginative accountancy. Certainly, each figure must be given a possible error of as much as twenty-five per cent; and that may well be charitable. My own belief is that it is much better to say that there is no meaningful method for computing these quantities.

Matters are made worse by the misleading way in which this table is printed. Part of it is reprinted as the table on the following page. Clearly, while the figures for the individual sectors may mean something, the figures for ' Total net imports ' and ' Total net exports ' mean nothing at all, unless Dr. Nevin really means that Textiles, Leather and Clothing had net exports in 1949 but large net imports in 1950. For the same reason, the idea that ' Total Welsh Debits ' rose from £150.0m. in 1948 to £305.7m. in 1952, and ' Total Welsh Credits ' from £130.2m. to £298.2m. is completely misleading. The figures for the earlier years are low largely because of paucity of information rather than paucity of transactions. Quite apart from this, the reliability of the residual is not very good. The item of —£7.5m. for 1952, which is called ' Net Investment abroad ', and has been obtained as the difference between £305.7m. and £298.2m. could easily be anything between + or —£150 million as far as the reliability of this table goes. It

	1948	1949	1950
Net imports of goods and services from other U.K. regions:			
i Chemicals	—	—	3.5
ii Textiles, leather and clothing	—	—	22.0
iii Other manufacturing	—	—	25.6
iv Food, drink and tobacco	32.4	39.4	57.5
v Other productions and trade	—	11.4	22.9
Total net imports from rest of U.K.	32.4	50.8	131.5
Net imports from overseas	97.7	117.8	100.4
Net exports of goods and services to other U.K. regions and overseas:			
i Agriculture, forestry and fishing	20.6	21.5	24.1
ii Mining and Quarrying	44.9	50.8	56.1
iii Chemicals iv Metals, engineering, vehicles v Textiles, leather and clothing vi Other manufacturing	32.1	49.8	106.7
vii Other production and trade	10.3	—	—
Total net exports	107.9	122.1	186.9

is a pity that so much political controversy has been born out of such a vague figure.

Now that we have examined the reliability of the tables on which it is based, we should be in a position to assess the worth of the revised Table I, which replaces the table in the BULLETIN. Unfortunately we cannot do this, because of a strange contradiction, arising once again from a peculiar conception of the residual error.

The margin of this table indicates that all of the items are derived either from some other table or from an independent calculation. Two items are printed in heavy type. These are ' Exports and Net Income received from Abroad ' and ' Imports '. The margin says quite clearly that they are derived from Table V (which may be why they appear in bold-type). And so we have the amazing result that Table I, which in the original had two residuals, one as an error and the other as income from abroad, on this occasion has no residual of its own. The two sides balance exactly although, according to the notes, they have been obtained independently. If this were to happen once I should be interested; but in fact it happens five times out of five.

The only possible explanation that I can think of is that the difference between the items ' exports and net income from abroad ' and ' imports ' has been obtained as a true residual of Table I. This difference has then been used to estimate the value of ' exports ' in Table V, after the ' imports ' have been obtained (or vice versa) and then the total broken down by some sort of input-output analysis. But this is just my theory: the book itself clearly indicates the contrary.

On the whole I do not think that the revision of these estimates necessitates any modification to my criticisms of the conclusions based on the earlier ones.

The purpose of this paper has been the examination of the validity of certain points made in Dr. Nevin's earlier paper rather than a complete review of his revised estimates. But it seems only fair to point out that, despite these strictures, the revised estimates contain some very valuable material which is about as accurate as it can be; the fault of the work is that, instead of being content with the solid achievements of tables such as Table VII, the editor attempted to calculate magnitudes which just cannot be calculated except with margins of error that make the estimates quite meaningless, and which a proper appreciation of the treatment of residuals would have prevented.

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A COMMENT

By EDWARD NEVIN

There are, I fear, a great many errors and misconceptions in Mr. Lewis's paper,[1] and the process of pointing them out, even briefly, will occupy a fair amount of space. Perhaps I may begin, however, by making some observations of a general nature.

First, the accounts which I constructed for 1950, and which were the basis of my article in this BULLETIN in February 1956, at no time made claims of accuracy. The first sentence of my article described the figures themselves as ' highly tentative and provisonal ', and it went on to say:

> It should be made clear that these estimates can at best indicate only broad orders of magnitude, and they represent little more than a basis for further work now proceeding along these lines at this college.

The estimates themselves were circulated in September 1955 in a document specifically described as a ' working paper ' in the covering letter sent with every copy. The letter also stated:

> As is made clear in the Introduction, the estimates which it contains should not be regarded as having any pretensions to a high degree of accuracy; they have been assembled simply to act as a basis and starting-point for a detailed and exhaustive investigation.

The Introduction to the estimates themselves stated:

> That the figures contained in the tables are frequently little more than mere guesses hardly requires stress, but the object of the exercise was not primarily to obtain precise information about the Welsh economy but to discover whether the construction of social accounts was or was not inherently possible.

It is a little difficult to see how greater emphasis could possibly have been given to the point, and I am mildly surprised that more than a year later someone should be flattering me by lengthy discussion of the methods then adopted to estimate various items. Looking back on these first estimates, I could myself subject them to much severer criticism than that to which Mr. Lewis charitably confines himself, but there really would seem to be very little point in taking up space on this occasion in an attempt to prove a degree of reliability for those estimates which neither I nor anyone else seeing the cautions quoted above would have dreamt of attributing to them.[2]

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[1] These, if I may say so, are not confined to his references to the work of my colleagues and myself. For example, the roneod document in the preparation of which Mr. Lewis collaborated with Miss Martin (*Welsh Economic Statistics*) was not published in 1954, as stated in Mr. Lewis's footnote 1, but in 1953. Again, Mr. Lewis indicates that the 1951 Population Census found that the excess of people living in Wales but working in England over those working in Wales and living in England was about 1400; in fact the correct figure is 4997 (Census 1951, England and Wales, *Report on usual residence and workplace*, H.M.S.O., London 1956, Table 6, p. 234).

[2] Although I must place on record the fact that there are a good many misconceptions in Mr. Lewis's discussion of these earlier figures; the main one—in connection with the residual error—is touched on at a later stage.

Insofar as Mr. Lewis seeks to carry over some of his criticisms to the estimates published by my four colleagues and myself in October 1956, however, some comment is called for and will duly be given.

The second general point is that the earlier working paper was circulated, according to its Introduction,

> with the sole purpose of obtaining comments on the general approach, on the usefulness of social accounts of this kind, and, it is hoped, suggestions as to how the individual estimates can be made more accurate.

My colleagues and I are sorry that in the months succeeding the circulation of these documents Mr. Lewis was not able to give us the benefit of the criticisms he now advances.

The third general observation is this. At several points in his discussion, Mr. Lewis expresses some concern at his inability to find in our statistical appendix a sufficiently full description of the methods of estimation used. The heading to this appendix states:

> It will be appreciated that space will not permit a full description of the large number of calculations involved, but the group will be very willing to provide a more detailed description of the statistical basis of any particular item or items in which the reader may be interested.

Once again, perhaps I may be allowed to express regret that Mr. Lewis did not think it worthwhile to take us up on this invitation in connection with the points on which he expresses mystification. I think this was rather unfortunate, since we might have been able to clear up several points for him, especially in connection with external transactions and the input-output calculations on which the misconceptions, if I may say so, are especially marked.

There is one other general point which I would like to make at this stage. On several occasions, and in several contexts, Mr. Lewis allocates ranges of error to items and then proceeds to add up the extremes to arrive at a total possible error. As is well known, I think, there is no justification whatever for this type of procedure in the context of social accounting estimates. In the first place, one is entitled, I think, to require rather more substantial authority for the arbitrary ranges of error which are postulated than some such phrase as ' a 5 per cent error is quite easily imagined ' or ' each figure must be given a possible error of as much as 25 per cent ' and so on. Further, as the compilers of the U.K. Blue Book have pointed out,[1] the error in any given social accounting aggregate is almost certain to be less than the weighted mean of the proportionate errors attaching to the components—firstly because errors are likely to cancel out, secondly because the aggregates are necessarily controlled by the relationship between income and expenditure, and thirdly because errors are likely to be constant when year-to-year changes (the findings of main interest in work of this kind) are being considered.

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[1] *National income: sources and methods*, Studies in official statistics, No. 3, H.M.S.O. London 1956, Chapter III, pp. 34–5.

Most important of all, perhaps, no-one imagines that the ranges of error of *any* economic statistics (with the possible exception of population figures) invariably lie within the kind of limits required for reliability by the text-books on statistical theory; a glance at the works of Morgenstern would soon disillusion anyone who did. Much less can such standards be applied to *estimates* based on such statistics, as is common knowledge amongst those having some experience of the practical handling of economic statistics for policy purposes in government or intergovernmental offices. All this is, so to speak, ancient history; it does not mean that all such estimates should be abandoned, or that they are useless, simply because any findings can be falsified by arbitrarily attributing a wide enough range of error to them. It is academic, in the very worst sense of that word, to reject a reasonably intelligent and objective estimate on the grounds that it *might* be quite different if we knew what in fact we do not know. The compilers of the U.K. national income statistics are, one supposes, amongst the two or three most expert groups of the kind in the world; yet they state quite bluntly:

> In present circumstances the estimator could not establish a system of national income statistics at all if he were compelled to restrict his presentation to those components in the system which are soundly based on accurately recorded facts.[1]

Hence, it is widely believed (no doubt correctly) that the possible error in the major aggregates in the U.K. Blue Books is probably greater in magnitude than the inflationary or deflationary gaps which commentators calculate from them. Does it then follow that the U.K. national income estimates should make no attempt to assess the magnitude of such gaps? This much is certain—if official statistics were confined to those which could be proved statistically to lie within a 10 per cent (or even 20 per cent.) range of error, the *Monthly Digest of Statistics* would then be able to slide easily into the ordinary business envelope.

Having made these general comments, perhaps I may deal briefly with the specific points which remain. There is no way of doing this other than that of commenting on the points *seriatim*; I apologise for the fact that this will inevitably make for rather arid reading.

1. *Structure of the Welsh economy*

Mr. Lewis begins by saying that ' Basically there is nothing wrong with this approach '; he then gives two reasons why it would be unwise to place too much reliance on the results obtained by it. May I point out that this is in fact a somewhat less emphatic repetition of what I myself wrote? I gave *three* reasons why this type of comparison has only limited meaning, and concluded that ' it would be unsafe to conclude, as a matter of practical policy, that a redistribution of the Welsh labour force along U.K. lines would necessarily raise the overall output per head.'

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[1] *Ibid.*, p. 33.

2. *Occupied female population*

Mr. Lewis states that I was wrong in suggesting that because of the ageing population 'the demographic trend will operate against an increase in the occupied female population' of Wales. But he then proceeds to say 'Now it is quite right that the Welsh female population is ageing, and that the number of women in the "active element" is going to decrease'. It is difficult to reconcile these two assertions. All that Mr. Lewis is trying to say, I think, is that an increased demand for Welsh female labour may *offset* this demographic trend; obviously this hardly disproves that such a trend exists.

3. *Revisions*

In effect the complaint here seems to be that in our revised estimates my earlier estimates have been revised. I really have no comment to make on this.

4. *Residuals*

Mr. Lewis states that only about a third of the residuals in our estimates are printed as such. Since we show six residual items, it follows that he is suggesting that we should have about eighteen residual items. As there are only seven tables in each of which one residual item could conceivably occur I fear we would find it difficult to oblige him in this regard. In stating that the undistributed income of companies is a residual in our estimates, I regret that Mr. Lewis is wrong. There can be no residual item in a set of figures derived by applying to a single total a percentage distribution which adds to 100.

5. *Employment income earned abroad*

Since the Census report to which Mr. Lewis refers appeared some considerable time after our estimates had gone to the printers, I hope we shall be forgiven for not having used the information which it contains. As Mr. Lewis would have discovered by enquiry, our estimate was based on an indication given by a well-informed official source.

6. *Remittances abroad* (*net*)

The answer to the series of questions on this point is that something is known about remittances into and out of the U.K. while nothing is known about remittances within the U.K. The way of the world is rather hard: having been roundly rebuked for making estimates on what are held to be inadequate data, we are now reviled for *not* making estimates for which there are *no* data whatsoever! (The question about Welshmen overseas, if I may say so, indicates a failure to appreciate the significance of the word 'net'.)

7. *Net investment abroad*

In his discussion on this point, Mr. Lewis is criticising a procedure which he *imagines* we followed ('this is just my theory'); since this procedure has very little resemblance to that which we in fact adopted, the long commentary on it has virtually no connection with our estimates, and I have no comment to make on it.

For the sake of the record, however, perhaps I may state once again that where items were derived as residuals they were printed in our tables in bold type; to make the position quite unambiguous (or so we hoped), they were so printed in the table in which they occurred as a residual, thus indicating to the reader not merely that they *were* residuals but also *where* they were residuals. When such an item was subsequently included in another table, it was not shown in bold type for the good reason that it would naturally not be a residual in that table. (The cross-references ensured that anyone examining such an estimate would realise that it was originally derived as a residual, of course.) Net investment abroad is clearly shown as a residual in our estimates in the external transactions table, where the type indicates without conceivable ambiguity that it is the residual item—it is, and can be, a residual nowhere else. There was nothing in the least mysterious about it, I fear.

One factor underlying Mr. Lewis's complete misconception on this point, if I may say so, is his practice of referring to the introductory discussion ' The interpretation of social accounts ' (he incorrectly refers to them in his paper as ' introductory notes ', of which there were none in our publication) as if that discussion purported to be a description of the methods by which the items in various tables were calculated. It is in fact nothing of the sort; even the title makes quite clear that it is a simple guide to the *interpretation* of social accounts in general, and I cannot believe that anyone reading the text could be under any misunderstanding about this. Bearing this in mind, the general theoretical statement which it contains to the effect that net investment abroad is the balancing item in the combined capital account would, I imagine, be perfectly unambiguous to the normal reader. Obviously it has nothing to do with the statistical processes of estimation.

8. *Exports and imports*

Mr. Lewis is quite wrong, I fear, in suggesting that our table (part of which he reproduces) shows a net export for textiles, leather and clothing in 1949. The figures he reproduces show plainly that in 1949 only a total for a set of four industrial groups was given. The statement that the 1948–49 values are low because of lack of data rather than lack of trade is correct; *he* is able to deduce this—why should we assume that no-one else would be able to?

What Mr. Lewis has failed to appreciate here is that the net balance of trade is obtained quite independently of the input-output breakdown, and is entirely unaffected by the degree of detail in that breakdown. The process by which the external transactions table was built up was one of some intricacy; it was naturally impossible for a detailed description of it to be compressed into the limited space of a statistical appendix. As I have already remarked, however, that appendix opened with an offer to supply full details of any calculation to anyone who might be interested in it. I am old-fashioned enough to regret that Mr. Lewis has indulged in rather dogmatic criticism of this table ('there is not a single reliable figure in it,' etc.) without having first extended to us the courtesy of an opportunity to explain to him how the table was in fact constructed.

9. *Residual, Table I*

Mr. Lewis's misconceptions over the residual in our Table I follow from this misunderstanding of the relationship between a residual balance-of-trade estimate in a GNP table and an input-output analysis which has to employ this, and other, social accounting entities in the final demand columns of its matrix. Such an input-output breakdown, of course, can achieve no independence of the basic GNP estimates on which it is based. His statement that my original Table I had two residuals, of course, is simply incorrect; he is failing to distinguish between, on the one hand, the allocation on a ratio basis of items (one of which is called 'residual error') from the U.K. Blue Book, and, on the other, the derivation of a residual in a separate GNP table for Wales. His statement that there is no residual in our current Table I is likewise incorrect; as the use of bold type clearly indicates to the reader, the trade balance is the residual in Table I *and nowhere else.*

Perhaps I may say in conclusion that no-one is more conscious of the inadequate statistical foundation of many of our estimates than are my colleagues and myself. Of course there are weaknesses and imperfections. But what is in danger of being lost to sight is that this work which we have begun for Wales (and which is similar to work going on in Scotland and Belfast) is an entirely new type of venture in this country. At the cost of a grotesquely presumptuous comparison, I would say that the present state of regional social accounting in the U.K. stands in the same relation to what will one day be achieved (through a combination of effort and constructive co-operation) as does Clark's *National income and outlay* to the 1956 issue of the National Income Blue Book. One can, if one chooses, make great play with the 'imaginative accounting' of *National income and outlay*; most of us, however, see it for what it was—a brilliant and pioneering work to which anyone using the current Blue Book estimates is hopelessly indebted. At the pioneering stage, one runs a serious risk of inviting mere destructive criticism (than which nothing, of course, is easier at such a stage) if one publishes, as we did, a reasonably full statistical appendix and makes no attempt to conceal the imperfections of the estimates. We were well aware of this, of course; but we hoped that something more positive and constructive would also result. In fact, we have received much constructive and perceptive criticism for which we are deeply grateful, and which will help us to continue the essential process of extension, revision and confirmation.

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FRESH FRUIT AND VEGETABLES STATISTICS IN BRITAIN

By L. G. BENNETT

This note is prompted by the article in the November BULLETIN of the Oxford Institute of Statistics in which Mr. George Allen draws some conclusions about horticultural marketing basing them partly on the available statistics relating to horticultural produce.[1] The purpose of this note is not to question the conclusions of that article but rather to point out some of the shortcomings of the background statistics on fruit and vegetable production, distribution and consumption and thus to put other workers in this field on their guard.

British agriculture has for long provided a wealth of statistical information on all aspects of production. Since 1939 the statistics on horticulture have become progressively more comprehensive and now cover not only the acreage of all crops grown, but also the production[2] and output[3] of outdoor vegetable crops and glasshouse crops and the production of top and soft fruit.

From time to time, the National Food Survey Committee of the Ministry of Food has published data on the consumption of fruit and vegetables in sample households in Britain[4] and we know from import statistics just what quantities of fruit and vegetables come into the country from overseas.[5] The 1950 Census of Distribution reports[6] provide, for the first time, very valuable data on the wholesale and retail trade in horticultural produce.

There is thus an apparently complete array of statistics on the production, supply, distribution and consumption of fruit and vegetables, and Allen uses some of these figures in order to estimate the magnitude of reconsignment, or double wholesaling, which is thought by many to be one of the more undesirable features of the fruit and vegetable trade. Two estimates of the extent of reconsignment are given, the first that it is about 35 per cent

[1] The reorganisation of fruit and vegetable wholesale marketing. BULLETIN, Vol. 18. No. 4, November, 1956.

[2] Production is used by the Horticultural and Price Statistics Branch of the Ministry of Agriculture in a restricted sense and means the quantity actually reaching marketing condition on the plants and trees or in the field. It is a term which is meant to cover the aggregate of all the subjective evaluations of yield made by a number of individuals.

[3] Output is used to denote the quantity actually leaving the holdings and entering into the flow of trade. The difference between production and output represents parts of plants discarded (such as outside leaves of cabbages) or parts of crops which do not, under prevailing conditions of demand, reach a sufficiently high standard to harvest.

[4] *The Urban Working-class Household Diet*, 1940-1949. H.M.S.O.
Food Consumption Levels in the United Kingdom, 1949. H.M.S.O.
Domestic Food Consumption and Expenditure, 1950. H.M.S.O. and since published annually.
See also Ministry of Health Advisory Committee on Nutrition, *First Report* 1937. Quoted by Denhardt, E. T. Food Control in Great Britain, I.L.O. 1942.

[5] *Trade and Navigation Accounts of the United Kingdom*. H.M.S.O.

[6] *Census of Distribution and Other Services*, 1950, Vols. II, III, H.M.S.O.

by value of the total wholesale trade and the second that it amounts to some £26 millions annually.

The method by which the first estimate is obtained is as follows:—

(*a*) The National Food Survey Committee gives the value of consumers' expenditure per head per annum in 1950 as £6.46.

(*b*) The Census of Distribution, Retail Trades, gives the retailers' margin as 18.9 per cent of the retail sale price.

(*c*) The wholesale value of retailers' purchases per head of the population per annum was therefore £5.23, and the total value £256 millions.

(*d*) The Census of Distribution, Wholesale Trades, gives the value of wholesalers' transactions as £396 millions

(*e*) The differences between £396 millions and £256 millions represents the extent of reconsignment.[1]

The method by which the second estimate is made is as follows:—

(*a*) The import statistics give the duty paid value of imported produce as £100.5 millions and this would appear twice in the wholesale trade.[2]

(*b*) Distribution costs amount to £10 millions.

(*c*) The value of home grown produce was £105 millions.

(*d*) This total of £316 millions falls short of the value of wholesalers' purchases by £26 million which must be accounted for by reconsignment.

The fact that two such widely differing results should have been obtained immediately throws grave doubt on their validity. Indeed, to estimate the extent to which undesirable practices are followed in the trade from data obtained from different sectors of it by different means, and therefore having different degrees of validity, may be very misleading and especially when the estimate is a residual factor. Moreover, two statistics are conspicuously absent. They are the quantities used in catering establishments, and the quantities wasted in the distributive process. This note, therefore, attempts to estimate the importance of these two factors and to show how dangerous it is to draw conclusions on the basis of the existing evidence. It will be wise, however, to examine the statistics themselves before attempting to draw any estimates from them and to make that examination in terms of quantity rather than value.

There can be little doubt that the import figures are reasonably accurate and in fact truly represent the volume of fruit and vegetables entering the country.

The Ministry of Food figures of consumption have certain admitted shortcomings. They are, for instance, confined to domestic consumption which means that 'meals out' and institutional consumption[3] are excluded and no figures appear to be available to indicate the quantity of fruit and vegetables of different kinds consumed in these ways. There is also a slight degree of unrepresentativeness in the sample households though the sampling is carried out on well established statistical principles.

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[1] This entirely neglects the direct grower-to-retailer trade.

[2] There is no evidence to show that it must necessarily appear twice. Some, in fact, would appear only once and some more frequently than twice.

[3] Institutional consumption is deemed to cover produce bought by canteens, hotels, restaurants and any other catering establishment.

It is quite probable that the Census of Distribution more effectively covered the wholesale trade than it did the retail trade because of the existence of a number of opportunist retailers of the ' barrow-boy ' type. Moreover, although the retail percentage mark-up is given in the Census report as 18.9 per cent of the retail sale value, investigators in this field have put it at nearer 33⅓ per cent.[1] Imperfect cover by the census would reduce the apparent size of the retail trade, and too low a percentage mark-up would increase the result of the calculation of the wholesale value of retailers' purchases.

Acreage figures on which the Ministry of Agriculture bases its estimates of production are fairly accurate but have one well-known shortcoming in that occupiers of holdings of 1 acre or under do not make either annual or quarterly returns.[2] While this is quite unimportant so far as agriculture is concerned, it may well have some significance in horticulture. One great merit which the Ministry of Agriculture figures have is that they are collected from every holding (with the exception noted above) in a known universe and were not, at the date of the Census of Distribution, drawn from a sample.[3] It is all the more regretable, therefore, that in proceeding to estimate the total supplies coming from horticultural holdings, the Ministry of Agriculture relies on a method which, useful though it may have been when introduced as a war-time expedient, is too rough and ready for any but the most limited purpose.

Very briefly, the method of crop estimating is as follows: In every county which is an important centre of horticultural production, there is a Horticultural Crop Intelligence Sub-Committee of the Agricultural Executive Committee. This Sub-Committee is convened by the County Horticultural Officer and consists of growers and country merchants chosen for their knowledge of the industry in their locality. The committee estimates the yields of crops being marketed and forecasts the prospects for those nearing maturity. These estimates are collated by the Horticultural and Price Statistics Branch of the Ministry of Agriculture, and form the basis of the estimates of total production.[4] They also form the basis for the calculation of the value[5] of home grown horticultural produce, a figure which plays a key part in Allen's second estimate.

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[1] Jefferys, J. B., *Distribution of Consumer Goods*.

[2] Glasshouse holdings of whatever size make twice-yearly returns of acreage.

[3] Since 1952 one-third samples have been used for estimating total acreage at three of the dates when quarterly returns are required.

[4] The Ministry of Agriculture production figures are for ' crop years ' and not calendar years, so that they are not strictly comparable with the consumption figures of the Ministry of Food. For an explanation of the system of yield estimating see *Economics of Horticulture*. (Papers read at a short course held in September, 1951, at the Agricultural Economics Research Institute, Oxford).

[5] The Ministry of Agriculture estimate of the value of home grown produce is the value of the produce only, *i.e.* it excludes packages and transport to market. It is quite different from the aggregate wholesale market values of the crops grown.

It is important to the horticultural industry that the supplies of produce reaching the markets be known with reasonable accuracy. For instance, the case for the exclusion of imports by tariffs or other means depends upon plentiful alternative home supplies. Again, the published average yields of many British horticultural crops fall far short of yields on well managed holdings and it is important to know just how far short they fall if any attempt is to be made to explain some of the economic difficulties with which growers appear to be faced. But in the present context it is perhaps even more important to know just what wastage of produce takes place between the holding and the consumer if the alleged inefficiency of the present marketing arrangements is to be fully comprehended.

Domestic consumption of fresh fruit has been given as 19.2 oz. per head per week, fresh vegetables as 13.8 oz. and other vegetables, excluding potatoes but including some dried and canned vegetables, as 18.1 oz. [1] This is a total of 51.1 oz. of fresh fruit and vegetables per head per week and is equivalent to a total annual domestic consumption of 3,705,000 tons. This quantity is made up partly of home grown produce and partly of imported produce. The home grown portion is obtained mainly by purchases in retail shops but also from allotments and gardens, the so-called ' free supplies ' because they are obtained other than by direct purchase. Free supplies amount to 425,000 tons per annum, but as they are irrelevant to the present discussion they are hereafter disregarded.[2] The total domestic *purchases* of 3,280,000 tons takes no account of the wastage in distribution and, as explained above, exclude institutional consumption.

The total quantity of fresh fruit and vegetables imported annually amounts to approximately 1,600,000 tons. In 1950 1,554,681 tons of imported fresh produce was contributed to the 3,280,000 tons of commercial supplies noted above.

Home grown supplies of fresh fruit and vegetables have been estimated at 3,234,637 tons.[3] This figure excludes potatoes, vegetables grown for drying or canning and excludes cider apples and perry pears. It also excludes the Scottish soft fruit crop which is grown mainly for processing.

The total quantity available to meet domestic purchases in 1950 of 3,280,000 tons plus institutional demand was 4,789,318 tons. The difference between apparent supply and known consumption was therefore 1,509,318 tons and this is accounted for partly by wastage and partly by institutional consumption.

Wastage in distribution is very difficult to estimate because the proportion wasted varies with the crop, with the time of the year, with the supply—demand relationship, is not confined to home-grown produce, and the point where it takes place influences the size of one of Allen's estimates of the extent of reconsignment.[1] One writer has put the wastage of vegetables due to discarding outside leaves, tops, adherent soil, etc., as 400,000 tons per annum of which the wastage on cabbages alone accounts for 200,000 tons. He also states that ' 16 per cent of the production of vegetables is thrown away between the farm and the consumers' table '.[2] A report dealing with the utilisation of agricultural products puts the total edible wastage of vegetables in 1949/50 as 98,000 tons.[3] Would it be right to add together these two estimates from different sources and thus arrive at total wastage, inedible and edible? The definitions of wastage given are not sufficiently clear to allow this to be done with complete confidence but it seems, on the facts stated, to be a reasonable course.

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[1] *Domestic Food Consumption and Expenditure,* 1950. H.M.S.O.

[2] But see *The Garden Controversy.* Best and Ward, Department of Agricultural Economics, Wye College, University of London.

[3] From holdings of over one acre.

Suppose then that wastage amounted to 498,000[4] tons in 1950. This means that one or other of two things must follow. Either that institutional consumption was of the order of 1 million tons (or nearly one-third of domestic consumption—which seems improbable), or that home supplies were over-estimated by nearly 8 per cent.[5]

Unsatisfactory as the situation appears to be at this stage, we can proceed to analyse the total figures into the main constituent items to get an even more unsatisfactory picture. The following table shows the relevant data for four kinds of produce.

Kind of produce	*Supplies tons*			*Utilisation tons*		
	Home Grown	*Imported*	*Total*	*Domestic Purchases* (*a*)	*Edible Waste* (*c*)	*Inedible Waste, Institutional Consumption*
Apples & Pears (*b*)	571,200	261,694	832,894	492,578	10,000	330,316
Tomatoes	121,000	188,094	309,094	335,156	3,000	—29,062
Peas and Beans ...	189,000	— (*d*)	189,000	155,971	12,000	21,029
Cabbages ...	723,000	— (*d*)	723,000	290,903	22,000	410,097

(*a*) Adjusted for free supplies. (*b*) Excluding cider apples and perry pears.
(*c*) Figures taken from *A Survey of Agricultural, Forestry, and Fishery Products.*
(*d*) Not separately shown in Trade and Navigation Accounts.

Little informed comment on the table above is possible, but attention may be drawn to some points which are revealed.

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[1] The greater part of the waste probably takes place at the retail stage. If this is correct, than the value of waste would increase the size of retailers' purchases and reduce apparent reconsignment.

[2] Pearson, C. E., ' Towards a more Stable Horticulture ' *Search,* November, 1951. Pearson's wastage appears to exclude loss due to physical deterioration and consequent disposal as refuse.

[3] *A Survey of Agricultural, Forestry and Fishery Products in the United Kingdom and their Utilisation.* H.M.S.O. 1953, Table X, p. 51.

[4] The 400,000 tons of Pearson plus the 98,000 tons given in *A Survey of Agricultural, Forestry and Fishery Products.*

[5] There is the ever-present error in estimate of home supplies due to the excluson of holdings of 1 acre or under. Whatever supplies, small though they may be, come from these holdings they either increase the degree to which home supplies are over-estimated or depress the figure for wastage.

Institutional consumption of apples and pears must be relatively unimportant as compared to domestic consumption and, moreover, apples and pears are not highly perishable. Are the yields of home grown apples and pears estimated at too high a level or is there some other outlet not accounted for?[1] Apple yields in 1950 were estimated at 64.8 cwts. per acre for dessert varieties and 101.5 cwts. for cooking varieties. Cider manufacturers use 100,000 to 125,000 tons of apples annually but these are mainly of cider varieties and only to a small extent do the makers use low grade dessert or cooking apples.

It appears that 29,000 tons of tomatoes are consumed which are neither grown in Britain nor imported, without taking institutional consumption or inedible wastage into account. Clearly the yield estimates or the domestic consumption estimates, or both, must be faulty.

The estimates for peas and beans agree fairly well. Fresh beans are much used by catering establishments and it is not unlikely that this outlet takes the greater part of the 21,000 tons unaccounted for.

Cabbages appear to provide the most glaring discrepancy but it has been stated earlier that 200,000 tons may be accounted for by wastage, leaving some 210,000 tons for institutional consumption. This would appear to be an improbable figure as it is not far short of that for domestic consumption. Either the yield has been overestimated or a proportion of the acreage, planned for human consumption, was in fact used as cattle food.

It is not possible to take the comparison of supplies and consumption any further than this, owing to the lack of information relating to some kinds of produce. Indeed, it might well be tedious to do so. An examination of the available statistics, however, has shown that they provide a very insecure foundation for general or for specific statements because they apparently leave 1½ million tons of produce out of account and a product by product examination has shown that serious errors might also well be inherent in some of the data. But on the quite unjustified assumption that the figures are reliable, then the value of this 1½ million tons could represent the whole of Allen's £26 million which is one of his estimates of the value of produce reconsigned from one wholesaler to another, and thus completely offset it. Moreover, if the cost of transport from the holding to market and the cost of packages are both added to the value of home grown produce as given by the Ministry of Agriculture[2] then reconsignment not only disappears but becomes a negative figure!

Any demonstration of the extent to which reconsignment takes place implies a charge of inefficiency against the marketing system which may, of course, be justified. For instance, a recent survey carried out in Birmingham market has shown that of 46 wholesale firms for which information is available, no less than 33 obtained the whole, and a further 10 a part, of their

[1] Some apples must be sold for jam making.

[2] These charges have been deducted by the Ministry of Agriculture from the wholesale market value in order to arrive at the net farm-gate value of production.

imported supplies from port merchants, *i.e.* reconsignment was necessarily involved.

There seem to be two methods by which the extent of reconsignment might be calculated given the necessary statistics. Reconsignment must equal

(*a*) the quantity handled by the wholesale trade *less* the sum of the quantity home grown and the quantity imported minus the quantity sold by growers direct to retailers, or

(*b*) the quantity handled by the wholesale trade *less* the sum of the quantity purchased by domestic consumers and by institutions, the quantity wasted in retail (but not wholesale) distribution and the quantity sold by growers direct to retailers.

But key factors to these and other similar calculations to do with marketing are either non-existent or of doubtful validity and attempts to estimate them have been unsuccessful. In these circumstances to base any conclusions about the extent to which reconsignment takes place on statistics drawn from the Census of Distribution, from Agricultural Statistics and from data given by the National Food Survey Committee can be a very misleading process.

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THE ECONOMIC CONSEQUENCES OF OVERSEAS TRADE CONCESSIONS

By PAUL STREETEN

The proposed tax remission on the profits earned by overseas trade corporations is intended to be, in the Chancellor's words, ' a step towards more investment, more trade, and more exports.' It echoes the theme struck in the opening sentence of his budget speech: ' With fifty million people living here upon a small island and dependent upon external trade and confidence in sterling it is this, the external position, which is always uppermost in the mind of any Chancellor '. During the oral evidence before the Royal Commission on Taxation, Mr. Millard (now Lord) Tucker asked Mr. S. P. Chambers (of Imperial Chemicals and the F.B.I.): ' . . . what I want is the unanswerable argument that British companies trading abroad should be relieved of their liability to tax . . . In fact, that is what everybody is talking about at the moment, increase your exports, and that is an export, the carrying on of business abroad, is it not? ' Mr. Chambers: ' Yes.'

Looking at the broad economic issues raised by this radical departure from British fiscal practice, two questions may be asked: (1) Is exemption an effective contribution towards the cure of our balance of payments ailments? (2) If effective, does the cure have harmful side-effects on the patient, which could be avoided if other remedies were used?

It is by no means certain that the proposed exemption will contribute to the solution of our recurrent balance of payments troubles. First, there is the problem of tax avoidance. Various arrangements between a parent company and its child (under-pricing sales and over-pricing purchases; routing orders that would have gone to the parent to its O.T.C. instead, etc.) would further erode, not merely the tax base (and throw a heavier burden on the remaining tax payers), but also our foreign exchange earnings. There would indeed be new scope for enterprise, initiative and ingenuity, in reshaping company structures to avoid profits tax on dividends, and in redefining overseas costs and profits so as to minimise tax liabilities. (All this may easily raise the loss of revenue substantially above the estimated annual £35m., particularly since anyone who sells exports is allowed to ' hive off ' an O.T.C.) This is perhaps not the kind of opportunity the Chancellor seeks to encourage. But such risks would have to be accepted if the economic case for relief were clear and strong. In fact, it is neither.

The second argument against exemption is that it will harm the foreign balance on *capital account*. Given the value of exports, an increase in foreign investment reduces the foreign exchange available for imports, repayment of debt, or addition to currency reserves. Between 1953 and 1956 we have been investing abroad at an annual rate of approximately £200m. This is over twice as much as our surplus on current account (excluding defence aid) over the same period. (See Table I). Although our current balance should not be accepted as a guide to what our net long-term foreign investment *ought* to be, it sets a limit to what it *can* be. The strain on the capital account will start at once and will continue as long as the concession lasts. This outpayment is bound to be greater than any receipts on current account in the short run. The Chancellor must therefore rest his case on the hope that foreign investment in British O.T.C.s. and the gains on *current* account in the long run (through a rise in receipts or a reduction in expenditure) will be sufficiently large to overtake the extra payments on *capital* account. But here again, the concession is a double-edged weapon: since the exemption penalises profits remitted home, we may also lose on current account through a reduction of profits receipts. (Item 5 in Table I.) The concession is therefore likely to lead to pressure on what the *Economic Survey* calls the ' commercial balance '.

Thirdly, the long-term effects may be detrimental to our foreign balance for still another reason. British subsidiaries abroad, benefiting from the research and experience of their parent concerns and from overseas sales outlets, may help, directly and indirectly, foreign countries to start and develop new products and sell at the expense of exporters without such subsidiaries, and also to become less dependent on our supplies. This may well by very desirable from many points of view, but it can hardly contribute to the solution of our balance of payments problem. There is no doubt that much American direct foreign investment has thus reduced demand for dollars. It is both paradoxical and sobering to remember that American firms have pressed and are pressing for identical relief on the ground that it would reduce the world dollar shortage, whilst British firms (and the Chancellor) argue that it would cure the sterling surfeit. Since foreign investment may ultimately increase certain types of foreign receipts (or at least prevent a greater fall) and reduce others, and since it may raise expenditure on imports as well as reduce it, there is no necessary contradiction between the American and the British claims. On the other hand, there is no general case for indiscriminate relief. It is least justified, from our point of view, when given to manufacturing concerns. The tax reduction may be passed on directly in lower export prices. It is difficult to see what else a ' hived off ' export company of a domestic manufacturer can do. It then amounts to an export subsidy—even if the commodities are bought free on board—whose wisdom or is questionable in lines where supply rather than demand limits exports, in which oligopolistic reductions by rivals are likely—not to speak of our international commitments. Alternatively, the exemption enables firms to accumulate larger reserves for expansion. Even then larger sales will tend to push down prices.

A valid reply would be that in countries where local conditions or policies make it inevitable that manufacturing will be done by some foreign subsidiary, the effective choice for us is whether it will be done by one of our firms' subsidiaries or by a foreign (e.g. German or American) one. But before we sanction this kind of salvaging operation we ought to be fairly

TABLE I

Net long-term overseas investment, current balance and overseas earnings, 1953–1956

(£ m)

	1953	*1954*	*1955*	*1956*	*Average annual rate 1953–1956*
1. Net long-term overseas investment, excluding[1] inter-governmental lending and repayment ...	180	200	110	140	158
2. Including inter-governmental lending and repayment	200	220	163	191	194
3 Current balance excluding defence aid	+86	+178	−123	+210	+88
4. Including defence aid	+188	+228	−79	+233	+143
5. Interest, profits and dividends received (net)[2]...	+74	+76	+63	+83	+74

Interest, profits and dividends received (net) as percentage of total imports 1929–37	*1953–56*
27	2.5

[1] *i.e.* deducting overseas disinvestment and long-term investment from overseas in the U.K. The figures include only some re-investment of retained profits.

[2] This item in general includes only investment income remitted to or from the U.K. Excludes overseas earnings and payments of U.K. insurance, shipping and oil companies.

Source: U.K. Balance of Payments 1945–1956 (No. 2.) Cmnd. 122.

certain that the reduction in export receipts would have been greater without the concession.

If the concessions enabled the firms to take the lead in economic progress, to move into the frontline of developments in international industry, the case for them would be much stronger. But evidence shows that tax concessions are too weak to achieve such revolutionary results.

Optimists might say that, even though foreigners may produce more for themselves and buy more from each other as an indirect result of our concessions, we should rely on the expansion of incomes and trade for our higher export earnings. Such hopes may have been justified in the past. To-day, many foreign countries plan to make themselves less dependent on our supplies as their incomes rise.

The answer, therefore, to the first question must be considerably more sceptical than that given by the Chancellor (' I am confident that this is a sound measure which will bring us increasing rewards in future years ') or that asked for by Lord Tucker.

If then the gains to our balance of payments are dubious, what sacrifices does this policy demand? There are at least two possible dangers to our real income: first, we may suffer a deterioration of our terms of trade; secondly, if overseas investment occurs at the expense of domestic investment, not only our export capacity would suffer, but also real income and our standard of living.

The first danger is absent if we encourage only mining and extraction, oil winning and agriculture, in short those commodities that we also import. Not only our balance but also our terms of trade may benefit. But the concession to manufacturing, processing and distribution may merely induce us to sell more at lower prices, without having achieved correspondingly higher productivity. If price reductions are greater than quantity increases, the policy is ineffective in improving the balance. If they are smaller, they will still involve a loss which we should set against the gains. It is true that most other methods of increasing exports would also move the terms of trade against us. But any method which encourages overseas investment means that we have a *bigger* balance of payments problem than we would have had if we had increased exports only. Total exports will then have to be greater for any given volume of imports, and hence the terms of trade will be worse.

The second objection to an indiscriminate concession to foreign investment can be illustrated by a simple—entirely hypothetical—example. Assume a firm considers a domestic investment project that raises the productivity of labour. Assume it could, as a result, sell goods worth 100, whose costs are made up of:

Raw materials	30
Wages	40
Depreciation	10
Total costs	80
Profits	20
Sales	100

If profits of more than 20 (say 25) can be earned in otherwise similar conditions abroad, the firm will invest abroad rather than at home. But from the nation's point of view this will be the wrong decision. Whereas the foreign profits of 25 represent the total gain to the nation (as well as to the firm), the domestic profits of 20 do not fully reflect the domestic gains. It would, of course, be false to add to profits the whole of wages because, in conditions of full employment, workers are taken away from the production of other goods. Their wages reflect partly the sacrifice of these alternatives. But only partly. Since the investment raises, *ex hypothesi*, the productivity of labour in the country and since workers in general (or possibly consumers, not considered in this example), will get a part (they may get the whole or even more) of this rise, 40 overstates the foregone production. Let us say it is 30. The domestic benefit to the nation is then 20 (profits) + 10 (excess of wages over foregone product) = 30. It is greater than the domestic private profit which guides private investment decisions. This constitutes an argument for discriminating against foreign investment, and *a fortiori*, against favouring it.

What, then, are the conclusions? The sound core of the case for exemption is that it is clearly desirable, from a national point of view, to prevent the emigration of managerial and technical skill and the loss of valuable trade connections which are the consequences of transferring management and control abroad. Beyond that, there is little to be said for encouraging overseas *investment* as such, though somewhat more in favour of encouraging *exports* (with a minimum of overseas investment), and most for encouraging the expansion of the production of *import*-type goods (mining, oil winning, agriculture). The concession does not distinguish between these objectives. It is likely that indiscriminate exemption will reduce our balance of payments. Even if it did not, it is doubtful whether the extra burden on the terms of trade and the sacrifice of domestic investment with higher returns to the nation, would be smaller than if we had adopted alternative, more selective, policies.

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A 'HUMAN NEEDS' DIET, SPRING 1957

By T. SCHULZ

This survey of the cost of a ' human needs ' diet for a family consisting of husband, wife, and three children of school age, was carried out in the last week of March and the first two weeks of April. The prices were collected in working-class districts in the following towns: London, Oxford, Reading, Birmingham, High Wycombe, Slough, Newbury, Abingdon, and Banbury. The total weekly cost of the diet was found to be 69s. 9d., *i.e.*, it was 1½d. above the cost of an equivalent diet in the autumn of 1956.[1]

The dietary to which this figure of cost applies is shown in the Table. It is somewhat different from the dietary on which we based our figure of cost in the autumn of 1956, in that the dietary for the spring of 1957 includes larger allowances of eggs and bacon, and smaller allowances of meat, herrings, and cooking fat, than the autumn dietary. Eggs are generally considered a semi-luxury food in a ' human needs ' diet for a healthy family with no infants, and Mr. Rowntree's ' human needs ' dietary for 1936 accordingly only included one egg per week for a family of five persons. In March and April of 1957, eggs were however exceptionally cheap, and it would have been reasonable for a housewhife catering on a small housekeeping allowance, to take advantage of this. We therefore included a dozen eggs in the weekly dietary; an even larger number might have been bought, in replacement of some more of the meat, or some of the bacon. Bacon, too, had considerably declined in price since the autumn, and boiling bacon, somewhat fat, but containing a proportion of lean meat, was offered at between 1s. 4d. and 1s. 6d. a lb. Boiling bacon without an appreciable amount of lean meat was sold at about 1s. a lb. We assumed that the family would spend 1s. 6d. on a lb. of boiling bacon and 1s. 2d. on half a lb. of streaky rashers. The increased amount of bacon in the diet would entail a decrease in the consumption of cooking fat, and we therefore reduced the amount of lard from 8 oz. in the autumn dietary, to 4 oz. Meat also was less dear in the spring than it had been in the autumn; nevertheless we left the average price unaltered, since a housewife who reduces her total weekly purchases of meat in favour of increased purchases of other first-class protein foods, might yet consider it a deterioration of the diet if she were to make the reduction in the family's *average* consumption of meat instead of in that of the cheaper cuts.

The main rises in prices occurred with respect to bread and sugar. The 1¾ lb. loaf of bread was generally sold at 10½d. in the autumn of 1956; in the spring of 1957 it usually cost between 11d. and 1s. Granulated sugar, costing 8½d. a lb. in the autumn, had risen in the spring to between 9d. and 10½d. The price of margarine—raised in our dietary from 1s. 6d. a lb. to 1s. 10d.—had in fact remained unchanged; but the cheaper kinds of margarine, at 1s. 6d. a lb., had in some places almost ceased to be available. Several shopkeepers averred that there was no demand for it.[1]

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[1] *Cf.* T. Schulz, *A ' Human Needs ' Diet: Autumn 1956.* BULLETIN Vol. 18 No. 4.

At the present time most customers dispose of incomes that enable them to choose the foods they want without regard to relatively small differences in cost. It is this trend in general demand that makes certain foods of the plainer kind noticeably inexpensive. Thus, every carcase contains a proportion of roasting meat and a proportion of meat suitable only for boiling or stewing; but while the demand for the more expensive roasting meat is high the lower grade boiling cuts tempt much fewer customers and hence are offered at prices that frequently are very economical. In March and April of 1957, roasting cuts of beef cost from about 2s. 8d. to well over 4s. a lb. At the same time, Argentine boiling beef could be obtained at from 10d. to 1s. 4d. a lb., and beef fat for rendering down at from 6d. to 10d. a lb.

Vegetables—in particular spring greens—fluctuated somewhat in price, and in order to secure the best bargain an economical housewife would have had to adjust her choice of a vegetable on any particular day in accordance with the supply of the market. The price asked for spring greens was on occasion 6d. a lb. This, however, was normally for greens of first-class quality and of which there was practically no wastage, so that a slight reduction in the quantity purchased would have compensated for the higher price without loss of nutrients.

It should be understood that the dietary presented in the Table is intended only as a picture of possible average consumption and not as a rigid standard to be adopted week by week. A housewife will scarcely buy every week, *e.g.*, exactly 1 lb. 12 oz. of cheese; nor, if she caters for a family of five persons, is she likely to buy only ½ lb. of herrings at a time. Also, cuts of meat and of boiling bacon will not be trimmed by the butcher to an exact weight and must be taken as offered. Adjustments and modifications of the dietary will therefore always be required in practice, and, indeed, the more economical a housewife in buying and the more skilful in catering, the more elastic will be the dietary adopted by her. Consequently, variations in the nutritional content of the diets provided by her are inevitable. These are of no importance if the composition of the dietary is satisfactory over a period of time.

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[1] That some of the low-priced foods were indeed in small demand, was indicated by the following observation: in one shop, which offered margarine at 1s. 6d. and Australian Cheddar cheese at 2s. a lb., eight customers were seen to buy Cheddar or Dutch cheese at between 2s. 4d. and 3s. 4d. a lb.; not one asked for the Australian Cheddar. Also, twelve customers—observed at two different times—bought margarine, but none of them asked for the cheap brand at 1s. 6d a lb. Both the margarine at 1s. 6d. a lb. and the Australian Cheddar cheese at 2s. a lb. were tested by us. The margarine was fresh and had a clean taste and the Cheddar had a good texture and a fair flavour.

Weekly 'Human Needs' Diet for 5 Persons—Spring 1957

	Quantity	Average Price per Unit[1]	*Changes since Autumn 1956*	
			Quantity	*Outlay*
		s. d.		*s. d.*
Meat	4 lb	2 4	*— 12 oz.*	*—1 9*
Bacon	1 lb 8 oz.	1 9½[2]	*+ 12 oz.*	*+ 5*
Herrings	8 oz.	1 0	*—1* lb	*—1 0*
Eggs	12 eggs	2	*+7 eggs*	*+ 4*
Cheese	1 lb 12 oz.	2 6	...	*— 7*
Milk, fresh	16 pt.	7½	...	...
do. condensed skimmed	2 tins[3]	10½	...	*+ 1*
School milk	5 pt.	free	...	...
Margarine	2 lb 4 oz.	1 10	...	*+ 9*
Other Fats	12 oz.	1 3½[4]	*— 4 oz.*	*— 3½*
Potatoes	21 lb	2	...	...
Onions	8 oz.	9	...	*+ 0½*
Other Root Vegetables[5]	7 lb	4	...	...
Greens	3 lb 8 oz.	5	...	...
Dried Legumes[6]	2 lb 8 oz.	11	...	...
Bread	19 lb 4 oz.	1 0	...	*+1 4½*
Flour	3 lb	1 7	...	*+ 1*
Rolled Oats	2 lb 8 oz.	10	...	...
Other Cereals[7]	1 lb 2 oz.	10¾	...	...
Sugar	3 lb	10½	...	*+ 6*
Tea	8 oz.	4 4	...	*+ 2*
Other Foods and Condiments[8]	...	3 7½	...	...
TOTAL COST		**69 9**		*+ 1½*

[1] The price unit is: eggs, one egg; fresh milk, pint; condensed milk, tin; bread, 1½ lb loaf; flour, 3 lb bag; other foods and condiments, total outlay.

[2] Average price per lb of 1 lb of fat boiling bacon at 1s. 6d. per lb and 8 oz. of streaky rashers at 2s. 4d. per lb.

[3] Equivalent to approximately 3¾ pt. of fresh skimmed milk and 11 oz. of sugar.

[4] Average price per lb of 4 oz. of lard at 1s. 6d. per lb and 8 oz. of suet at 1s. 2d. per lb.

[5] Carrots, swedes, turnips, beetroot.

[6] Haricot beans, lentils, peas.

[7] Small grain rice, macaroni, semolina, barley.

[8] Cocoa, jam or syrup or dried fruit, salt, vinegar, etc.

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